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FLUCTUATIONS IN THE ABUNDANCE OF THE VARIOUS YEAR-CLASSES OF FOOD FISHES

REPORTS PREPARED BY SPECIAL REPORTERS NOMINATED BY THE COUNCIL AND INDICATING THE MAIN RESULTS BROUGHT OUT BY THE PAPERS READ AT THE BIOLOGICAL MEETING OF LONDON, IN 1929

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PREFACE

The present volume is devoted to a further discussion of the important problem of Fluctuations, which was considered in some detail at the London meeting on April 12th, 1929 (see Rapports et Procès-Verbaux, Vol. LXV, Fluctuations in the Abundance of the Various Year Classes of Food Fishes, Copenhagen, 1930). In order to focus the discussion on points of practical importance the suggestion was made that this year's meeting should consider first of all the methods of determining fluctuations, and should then formulate plans for the organised and continuous study of these fluctuations and their causes. As each fish presents special problems and requires special methods of investigation, it seemed best to treat each of the principal species separately, and accordingly a Reporter was appointed for each of the following species — cod, haddock, plaice, herring, sprat and sardine. The Reporters were asked to give particular attention to the following questions:—

"I. Validity of the Methods in Use. This should cover a consideration of the adequacy of sampling, tests of adequacy and consistence, possibility of determining mortality rates, and methods for such determinations.

II. Organisation of Regular Co-operative Studies on Fluctuations. A regular system of "Biological Statistics" regarding the principal species of fish is essential for the Council's work. Plans for the extension and improvement of the present work on this subject should be considered, in relation to the problems (1) of making reliable forecasts of future yield, and (2) of throwing light upon the relation between fluctuations in different species, and in the same species in different areas.

III. Investigation of the Causes of Fluctuations. It is important to establish at what stage in the life-history of each species the magnitude of the year-group is mainly determined, and to study the conditions — biological and hydrographical — that affect survival. The methods of studying these conditions should be discussed, together with plans for their co-operative investigation."

Their reports are here printed in full, together with a somewhat incomplete account of the discussions which followed.

E. S. Russell,
Convener.

I. COD

Notes for Discussion of the Fluctuations in Abundance of Year-classes of Cod (*Gadus callarias* L.) in European Waters.

By

Michael Graham.

At the biological meeting of the International Council for the Exploration of the Sea held in London on April 12th, 1929, there were delivered twenty papers on fluctuations in abundance of the year-classes of fishes, of which four papers were concerned with the cod. The present notes attempt to present the matter of these papers in relation to the problem as a whole, introducing, where necessary, other relevant matter, even though not mentioned in the four papers delivered last year. It is proposed that these notes should, however, be concerned strictly with fluctuations in the year-classes, and not extended to the consideration of fluctuations in the yield of the fisheries. In general we believe, owing largely to the work of Hjort and his colleagues, that fluctuations in abundance of the year-classes do dominate the yield of the fisheries; but there are strong suggestions in the work of Le Danois, Du Baty and Beaugé, that the yield of cod on the Newfoundland Banks is dependent mainly on the physical conditions prevailing at the time of the fishery[1]). It is well, perhaps, to forego discussion of these suggestions by French workers, as being outside the scope of the present meeting, since in European waters it appears that fluctuations in the year-classes do govern the yield, and our time is limited. We may also note, in passing, that there is available a large amount of information on fluctuations in the yield of the fisheries, which we may ultimately hope to interpret in terms of fluctuations in year-classes. No better example of this material can be chosen than that of the Aberdeen trawling statistics, so clearly expounded by D'Arcy Thompson last year[2]).

Our discussion falls under three headings:— I. The validity of the methods of estimating the fluctuations; II. Proposals as to a programme for routine statistics of relative abundance of year-classes; III. Investigation of the causes of these fluctuations.

Before going on to consider the subject under these headings, it is well to observe

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[1]) e.g. Beaugé, L. Rapport de Mission sur le Banc de Terre-Neuve (Campagnes 1927, 1928). Rev Trav. de l'Office de Pêches Marit. Tomes I, II. Paris 1928, 1929.

[2]) Thompson, D'Arcy W. On Fluctuations in the Abundance of Cod. Cons. Int. Expl. de la Mer. Rapports LXV. p. 18. Copenhagen, 1930.

that the cod is spread so widely over our area, that the problem of fluctuations in different parts of the area cannot be studied by one uniform method. In Fig. 1 is shown an approximate picture of the distribution of cod in European waters. Three classes of density of cod are distinguished: "Abundant", "Common" and "Scarce". The distinction of different parts of the area according to these three classes of abundance is derived from a study of the English and International Statistics for 1927. The methods of ap-

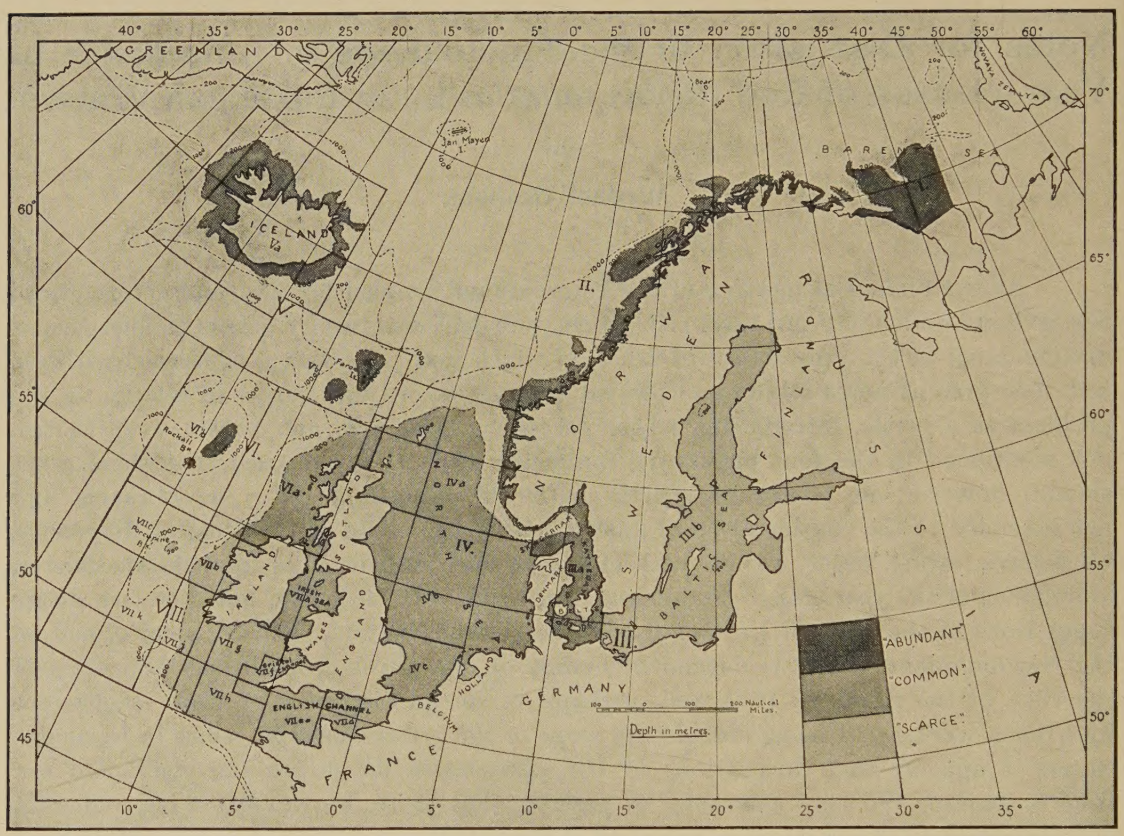

Fig. 1. The Density of Cod in the Fishing Regions of Europe. The distinction of three classes of density, "Abundant", "Common" and "Scarce", is made from the English and International Statistics of 1927; which agreed in so much distinction, in all regions where duplicate information was available, except Rockall, the classification of which is doubtful. It is also clear that there must, in fact, be gradation in the Skagerak to Baltic Sea area. — The assumption is made that the bulk of the cod are taken in water of less depth than 200 metres.

proximation that have been used will be described in a paper that is in preparation for the English series of reports, and, since there is nothing contentious in the findings of the chart, I need not describe them here. (It should be noted, however, that there is some doubt about the classification of the Rockall region, and that there must, in fact, be gradation in the Skagerak-to-Baltic-Sea area, which shows an improbable discontinuity, in the statistics used). If we consider the chart of Fig. 1 in the light of general

geographical and fishery knowledge, we must conclude that the problem will have to be considered separately in the following areas:— Barents Sea to the Kattegat; North Sea, Scottish Waters and Irish Sea; Iceland; Faröe Islands; possibly Faröe Bank; Rockall. (As is well-known, there is evidence that still further sub-division of stocks is desirable in practice; but precise definition of such sub-divisions is unnecessary at this stage). The Norwegian and North Sea stocks may be cited, as an example of two areas in which the cod must live in different ecological conditions; and in which fishery practice also imposes different sampling technique by the investigator. We understand that cod in Norway are already selected in Nature into enormous groups (löddefisk, skrei etc.) having in common some biological characteristic, such as pursuit of *Mallotus*, or ripening for breeding; and these biological groups each form the object of a more or less self-contained fishery. Cod in the North Sea, on the other hand, are only sorted in Nature into small biological units, shoals, which are distributed more dispersely, so that the catch of the fisherman is more rarely composed of a single biological group, and the catch of a fishing port is nearly always heterogeneous in this respect.

To proceed with heading I, the validity of the methods in use in estimating fluctuations in the year-classes of cod; we may consider two different classes of methods: (a) observations on marketable cod, and (b) observations on pelagic fry. At the present time method *a* has been more commonly used than method *b*. Estimation of fluctuations of year-classes by observations on marketable fish involves the use of age-determination technique, but there is an important difference in practice, according to whether the age-determination methods may be used throughout the life of the fish, or whether the age-determination technique is only applicable in early years. Where otoliths or scales may be safely read throughout most of the life of the fish, intermittent sampling, provided it is sufficient to determine the age-composition of the stock at the time, will keep track of the fluctuations from year to year. By this method the densities of the year-classes are not expressed in relation to fishing power, but are measured relative to the density of each other. But, when age-determination methods may only safely be applied to the youngest fish, it becomes necessary to determine every year the density of the stock of these young fish, relative to some constant factor, such as a unit of fishing power. It is therefore necessary to take careful stock of the position with regard to the reliability of age-determination technique in the cod. It behoves us to be particularly careful, since opinion in this matter has changed during the last few years. Today we find that, in the North Sea, the cod scale has only been found reliable for the first three years of life, and only then by disregarding the misleading information given by a proportion of the scales[1]). For Norwegian cod, also, we learn, from Sund, that "the information obtained from scales or otoliths is apt to be seriously blurred by personal bias as soon as the age of the fish is, say, 6 years or more"[2]). This statement of Sund's must mean

[1]) Graham, M. Studies of Age-determination in Fish. Part I. Fish. Invest. II. XI. 2. London 1928.

[2]) Sund, O. The renewal of a Fish Population studied by means of measurements of Commercial Catches. Example: The Arcto-Norwegian Cod Stock. Cons. Int. Expl. de la Mer. Rapports LXV. pp. 10—11. Copenhagen 1930.

that the structure is indefinite in fish of 6 years old or more, since personal bias could not affect observations of a definite feature. To those of us who have had experience of scale-reading, it is evident that these older scales are unsuitable owing to feebleness in the marking of winter-rings, or to adventitious rings being equally clearly marked as winter-rings, or both phenomena; for it is thus that scope is provided for personal bias. It may therefore be accepted that, for two stocks which have been very thoroughly studied, those of Norway and the North Sea, age-estimations of cod may only be made for the youngest marketable fish. In the light of this finding it is probably well to regard with considerable reserve the opinions evidently held by WINGE[1]) and SAEMUNDSSON[2]), that scales or otoliths are reliable over a longer range of age, in Faröe and Iceland cod, respectively.

Before further discussing the use of age-determination technique in collecting statistics of the year-classes, we must make certain that there is good evidence of the reliability of the technique in the cod's early years, in the North Sea and in Norwegian waters. In the North Sea we may attach to a trawl a cover-net of sufficiently fine mesh to take the smaller sizes of cod in approximately the proportion in which they occur on the ground. By sufficiently examining the lengths of cod that are present in the sea at different seasons, we may, by the well-known method of PETERSEN[3]), determine the age of the smaller length-groups[4]). These determinations may be used to confirm our readings of scales or otoliths, in the early years during which the lengths remain grouped, and a little farther, by extrapolation of the growth-curve. In this way such general agreement has been found in the North Sea, that we are justified in using the scales to determine the age of the majority of fish in any sample of three years old or less age, provided the sample is found to consist mainly of fish of one single year-class, and also in cases where length measurements justify the distinction of more year-classes[5]). The chance of obtaining an incorrect majority-reading for the whole sample, is, on theoretical grounds very small, but, owing to shoaling in length-groups[6]), it is probably well to examine a number of separately-collected samples of any population under review. (In the course of work on which I hope to report very soon, I have found that quite small samples of fish, of about 50 specimens, give very consistent results in determining the age of the youngest marketable codling in the North Sea in 1929, throughout 22 separate samples). For checking the validity of SUND's determination that the youngest

[1]) WINGE, O. On the Value of the Rings in the scales of the Cod as a means of Age-determination. Medd. Komm. Havundersøgelser. Fisk. IV. 3. Copenhagen, 1916.

[2]) SAEMUNDSSON, B. On the Age and Growth of the Cod (*Gadus callarias* L.) in Icelandic Waters. Medd. Komm. Havundersøgelser. Fisk. VIII. 3. Copenhagen, 1923.

[3]) PETERSEN, C. G. J. Papers on the Biology of Flat-fishes. Reps. Dan. Biol. Sta. Copenhagen, 1892, 1894, 1896.

[4]) e.g. THOMPSON, D'ARCY W. Second report on the Distribution of the Cod and other round fishes. Cons. Int. Expl. de la Mer. Rapports. XIII. 2. Copenhagen, 1911.

[5]) GRAHAM (1929) *loc. cit.* p. 41.

[6]) RUSSELL, E. S. Market Measurements in Relation to the English Cod Fishery. Fish. Invest. II. V. 1. p. 54 par. 2. London, 1922. Also: HJORT, JOHAN. Introductory Address. Cons. Int. Expl. de la Mer. Rapports LXV. p. 7. Copenhagen, 1930.

löddefisk are 4 years old, there is the same sort of evidence, quoted by HJORT[1]), from the data of DAMAS[2]). With regard to the III-group, the table of DAMAS (p. 120) comprises 262 fish in all, 210 in the II- and III-groups, and the lengths of the II- and III-groups are not very well separated. DAMAS also gives data (pp. 108, 112, 117) of three samples caught by line or seine, which also support his interpretation of the scales. I have been unable to find any other published evidence of this kind. While, therefore, the evidence, so far as it goes, supports SUND's estimation of 4 years old as the age of the youngest marketable fish, it would be comforting in so important a matter, upon which the dating of the Norwegian fluctuations entirely depends, if SUND's estimation were supported by more extensive data of the same sort. Four years ago it would, perhaps, have been presumptuous to have made this suggestion, but today we are less certain of our ground, for we know that older Norwegian scales are unsuitable for age-determination, and even young North Sea scales may be actually misleading. It is very important that there should be no Achilles heel in this magnificent Norwegian research, which, if any can do so, should provide us with the key to fluctuations in the fisheries. At the present time a controversially-minded person might say that he had concluded that the youngest marketable Norwegian fish are two years old, having put on two winter-rings per annum, and it would not be easy to prove that he was wrong.

Fortunately, the researches of HJORT and SUND have shown that the fluctuations in the year-classes of the Norwegian stock are determined prior to the attainment of marketable size. This is admirably shown in SUND's Fig. 6[3]), where the fluctuations in the smallest fish are always reflected in later years in fish of larger size, with the one exception of the abundant fish of 40 cms. and less length recorded in 1913. The same fact is shown by different statistical methods, in RUSSELL's Fig. 7[4]), for the North Sea cod. I have continued RUSSELL's figure to the present date, and his finding is confirmed, Fig. 2. In Fig. 2 there is plotted the average annual density of the three statistical categories of cod recognised in English statistics, density being measured by the catch per day's absence of all 1st Class British steam trawlers landing their own catches in England from the North Sea. The categories are distinguished according to size of fish[5]). In drawing Fig. 2 a suggestion made by Professor D'ARCY THOMPSON has been followed, and the lines for the categories have been moved so that their courses are roughly parallel. It is to be observed that there is close correspondence between the course of the line representing "Small" in any year and the line representing "Medium" in the following year. The fluctuations are, however, somewhat damped. In the "Large" category the fluctuations are damped still more, so that there is some doubt whether

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[1]) HJORT, JOHAN. Fluctuations in the Great Fisheries of Northern Europe. Cons. Int. Expl. de la Mer. Rapports XX. p. 129 *et seq.* Copenhagen, 1914.

[2]) DAMAS, D. Contribution à la Biologie des Gadides. Cons. Int. Expl. de la Mer. Rapports X. 3. p. 117 *et seq.* Copenhagen, 1909.

[3]) SUND (1930) *loc. cit.*

[4]) RUSSELL (1922) *loc. cit.*

[5]) RUSSELL (1922) *loc. cit.* p. 6. The Grimsby sorting is the most important, since the Grimsby catch predominates over that of other ports.

the line for "Large" should be placed two years differently from "Medium", or whether a three-year interval would not give as good correspondence. The conclusion appears inevitable, from the correspondence of the graphs, that the fluctuations in the yield of the North Sea cod fishery are already determined by the time the cod reaches marketable size, as Sund has found in Norway. Complete interpretation of Fig. 2 would lead us far beyond our available time, but there is a small step in interpretation that we are justified in taking even now. This is to restate our main observation in terms of year-

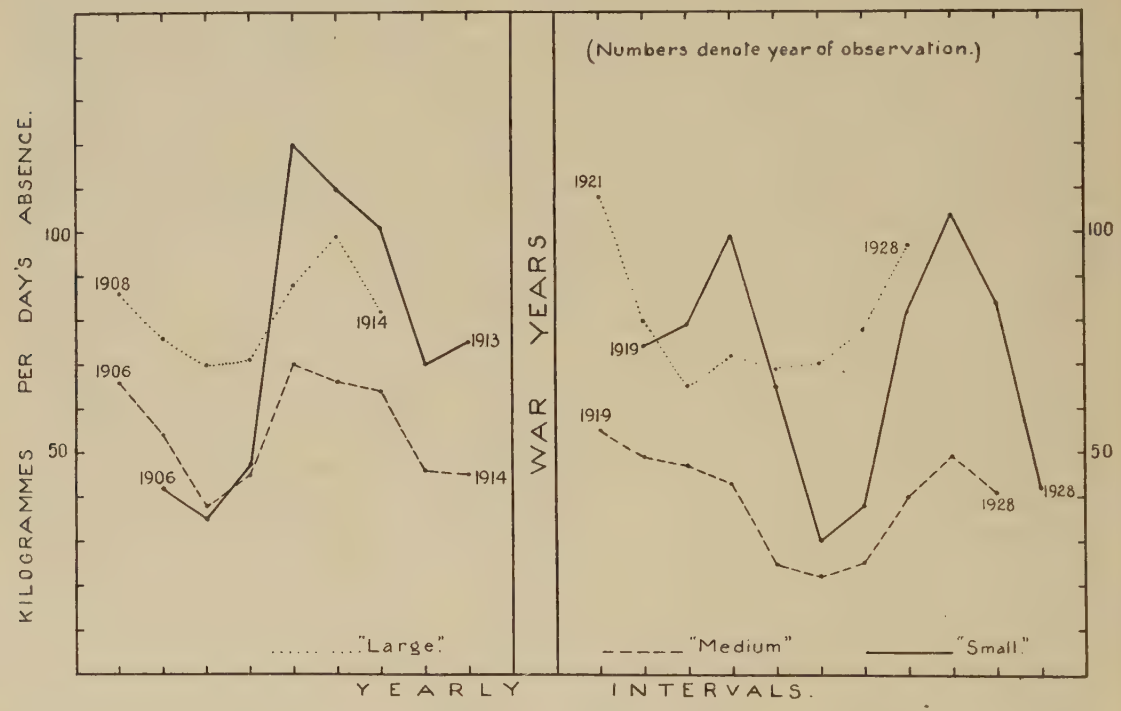

Fig. 2. Fluctuations in the Density of Categories of Cod recognised in English Statistics. — The vertical scale represents the annual average catch per day's absence of British 1st Class Steam Trawlers landing their own catch from the North Sea in England and Wales. Three size categories of cod are distinguished, and the diagrams for these three categories are so placed, over the horizontal scale of yearly intervals, that the catch of Medium in any year is in vertical line with the catch of Small in the preceding year, and the catch of Large is placed with a 2-year interval from Medium. The significance of the parallelism so obtained is discussed in the text p. 13.

classes. It is reasonably certain that interpretation of Fig. 2 will show that the fluctuations in the year-classes also, are already determined at the age at which the cod first becomes marketable. It is hoped to publish, in the near future, research on the growth-rate of codling in the North Sea, which will enable a complete interpretation of Fig. 2 to be made in terms of brood-years. We shall then be able to give comparative estimates of the yield of all the brood years in the period shown; but for this purpose this particular figure, in which the statistics are arranged in calendar years, and the landings at all

ports are considered together, is probably less suitable than a figure based on some rearrangement of the statistics. In Fig. 2 in its present form, however, we may ascribe the brood years to the extreme values on our graph of "Small", with little chance of error, extremely high, or extremely low values of "Small' in any calendar year being the result of very rich or very poor broods two years previously. On this reading, 1905 and 1921 were years of poor brood, and 1907, 1919 and 1924 were rich year-broods. There is every prospect of carrying the interpretation still further, so as to include also years of moderate brood-production. (The interpretation depends on several factors remaining sufficiently nearly constant. These are:— the growth-rate for the principal codling areas of the North Sea, the sorting on Grimsby market [as converted into statistical categories by our market staff], the preponderance of fishing from Grimsby over other fishing in the North Sea, and the adequacy of sampling of the codling areas by Grimsby fishing). We have therefore, in these statistics, the necessary records of abundance of year-classes in North Sea cod. At the same time it is to be observed that, in both the Norwegian and the English series, there will always be a small element of doubt, due to the possibility that growth may be abnormal in particular years, so that the age of the youngest marketable fish is greater or less than we assume it to be, from our determinations of the normal growth-rate. It is interesting that the application of SUND's method to the North Sea cod fishery has so far not proved practicable. It appears impossible to obtain sufficiently representative length measurements of the North Sea stock of cod. Partly this may be due to the less density of cod in the North Sea (Fig. 1), aggravated perhaps by a tendency for small and mediumsized cod to frequent rough grounds, where there is some protection from our trawlers, but less protection from the baited lines of fishermen in Norway.

We have seen that we may probably accept the view that fluctuations in the year-classes are already determined before the cod reaches marketable size. There is also some evidence that the fluctuations do not depend on the number of spawners[1]), in other words, that the fluctuations are determined between the egg-stage and the attainment of marketable size by the fish. For some time past there has been a suspicion that the critical phase is of even shorter duration, so that the fluctuations are already determined in the number of pelagic fry. Such was certainly HJORT's view[1]), and POULSEN now promises us strong correlation between the numbers of pelagic fry in any year and the number of demersal stage young cod in the same and subsequent years[2]). It is regrettable that the evidence for POULSEN's statement has not yet been published, for his finding is of the very greatest importance in the problem before us. In the paper cited POULSEN shows good correspondence between the numbers of pelagic fry in the Kattegat and the Belt Sea in six years, and between the number of fry in the Belt Sea and the temperature and salinity. The correlation, for which his figures give evidence, is all the more remarkable in view of the scanty nature of the data (for instance

[1]) HJORT (1914) *loc. cit.* p. 204.

[2]) POULSEN, E. M. On the Fluctuations in the Abundance of Cod Fry in the Kattegat and the Belt Sea and Causes of the Same. Cons. Int. Expl. de la Mer. Rapports LXV. p. 26. Copenhagen, 1930.

only 4 hauls in the Belt Sea in 1924), and also since POULSEN has made no allowance for mortality of fry, which were presumably not all at the same stage in every year. We can only believe that better sampling and more refined treatment of the material would result in still better correlation, because sampling errors and error due to mortality would normally militate against correlation being found. There need be no surprise that POULSEN did not use a closing device on his ring-trawl, nor one of the more elegant "quantitative" plankton nets, such as that of HENSEN; for GARDINER and I concluded, in a slender paper (which has perhaps not received the attention it deserves) that even PETERSEN's Young Fish Trawl, used obliquely, without closing, will give results just as "quantitative" as the nets of greater refinement. It appears that "patchiness" in the planktonts is the main cause of variation in catches, and that, therefore, a large net, such as PETERSEN's or the Ring-trawl used by POULSEN, gains in precision from its size as compared with the so-called "quantitative" nets, quite sufficiently to compensate for lack of a closing device[1]). The point, admittedly, requires confirmation. Before going on to consider the implications of POULSEN's correlation between fry and young demersal stages, it may be well to pause, in order to consider a general feature of the Transition Area. We must observe that POULSEN's is not the only correlation that has been found in this area; for example, JENSEN shows good correlation between the cod and herring fisheries of the same waters[2]). In fact, in comparison with other areas that we know, the Transition Area might well be renamed the "Correlation Area". It may well be, of course, that similar correlations will be found in other waters; but the possibility remains that the Transition Area may be peculiar in the arrangement of its environmental conditions, so that the fisheries are abnormally liable to be affected by the hydrography of the area. Should this be the case it is probable that many phenomena will be causally connected with some dominant physical factor, in which case a number of pairs of phenomena may show strong correlation, although there may be no causal connection between them; but both may truly depend on a third phenomenon. Nevertheless, the line of research that POULSEN has followed promises so many advantages (which we may consider below, under heading II) that it seems very desirable that similar work should be put in hand in other areas.

Some review having now been made of the methods actually in use, we may turn to heading II, the discussion of an actual programme for statistics of abundance of the year-classes of cod. As a basis for discussion I make certain concrete proposals. The proposals come under two headings:—

a) The estimation from year to year of the abundance of youngest marketable fish, and assessment of their age,

b) The estimation from year to year of the abundance of pelagic fry of the cod, and, for the next few years at any rate, correlation of the abundance of pelagic stages and the youngest marketable fish.

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[1]) GARDINER, A. C. and GRAHAM, M. The Working Error of Petersen's Young Fish Trawl. Fish. Invest. II. VII. 3. London, 1925.

[2]) JENSEN, A. J. C. On the Influence of the size of the Stock of Cod upon the yield of the Herring Fishery. Medd. Komm. Havundersøgelser. Fisk. VII. 8. Copenhagen 1929.

In years to come we may hope to discontinue *a* in favour of *b*, but it is unlikely that method *b* can be placed on a sound basis within, say, the next ten years. The advantages which would accrue from the adoption of method *b*, estimation of pelagic fry, rather than estimation of youngest marketable fish, appear to be four in number:—

I) Emancipation from age-determination technique and the assumption about normal growth-rate, which form a slightly doubtful basis.

II) Applicability to areas where our statistics may not be sufficiently reliable for the application of method *a* (Iceland etc., see below).

III) The consideration that forecasts would be available earlier, from observations of the fry, than they would be from the marketable fish, which appear from 2 to 4 years later.

IV) The fact that cruises of research vessels, such as would be necessary for method *b*, are in any case required for investigations of the causes of fluctuations. (See below under heading III).

With regard to method *a*, the estimation of fluctuations by observation of the youngest marketable fish, I propose that the system already applied for the North Sea and Norwegian stocks should be continued. It is also desirable that the observations should be extended to other regions; but it seems that we shall have to consider Rockall, Greenland[1]) and Bear Island[1]) as out of our effective reach, at any rate by this method. The problem at Iceland and the Faröes has some features in common: in both regions there are English trawl statistics of catch per day's absence by categories, but it has not yet been shown that the trawlers sample the stock adequately and in a uniform manner. In both regions also, there is a line fishery near the shore. It is therefore theoretically possible that either Sund's method, or the English method, or both may be found to be applicable; but it seems doubtful whether the trawl statistics will be found to sample these regions with sufficient uniformity, from year to year, as regards different sizes of cod; and I do not know whether it would be practicable to arrange such extensive measuring of the inshore catch as is required by Sund's method. In both cases adequate data of the age of the youngest fish are needed for the interpretation of the statistics. At Iceland this seems a very difficult matter to determine, owing to the differences in growth-rate on the different coasts[2]), and it cannot be judged here whether the problem of interpreting statistics in terms of age is amenable to treatment or not: the matter requires special investigation. As regards the Faröe region, also, there appears to be the complication that the rate of growth is greater on the Faröe Bank than it is round the islands[3]), the stocks of which appear to be mainly separate[4]). It is evident, how-

[1]) Large scale fishing has only recently reached Greenland and Bear Island, which were, therefore, not considered in Fig. 1.

[2]) Saemundsson (1923) *loc. cit.* p. 28.

[3]) Schmidt, Johs. Cons. Int. Expl. de la Mer. Rapp. et Proc.-Verb. XLIX. p. 128. Copenhagen 1928.

[4]) Strubberg, A. C. Marking Experiments with Cod at the Faroes. Medd. Komm. Havundersøgelser. Fisk. V. 2. Copenhagen, 1916.

ever, from the papers cited, that in both regions, Iceland and Faröe, there is sufficient information available as to the growth-rate of young fish in different localities. The doubtful point is whether any statistics will consist of one growth-type, with sufficient homogeneity. In Scottish waters, excluding the North Sea, there would be required a long investigation of average growth-rate of the young fish, before any interpretation of statistics in terms of age could be made. In the Transition Area it appears probable, from the papers of Strubberg[1]) and Jensen[2]), that the estimation of the abundance of the youngest marketable fish, and determination of their age, could be made, in spite of the complications of the presence of two stocks, indigenous and transitory, and the diversity of fishing methods. In the Baltic also it would appear from the study by Hessle[3]), that the stock could be brought under sufficient observation.

With regard to method *b*, observations on pelagic fry, my proposal is that there should be a concerted programme, designed, firstly, for the standardisation of technique, and, secondly, for investigation of whether there is, or is not, a clear correlation between the number of pelagic fry (or the direction of their passive transport) and the subsequent yield of the fisheries. As regards technique, we should, first, test the adequacy of the 2-metre ring-trawl, used obliquely (10 minutes near the bottom, 10 minutes in midwater, 10 minutes near the surface) or by some other method, as a sampling instrument. Then, mortality rates of different stages of fry must be determined, so that observations may be comparable, even when not made on fry of same stage of development. Treatment of the data also requires investigation, so that we should have some method analogous with that of Wollaston[4]), by which we may convert our observations into a value proportional to the total population. My proposal is that all the countries concerned should undertake work on these lines.

With regard to heading III, investigation of the causes of fluctuations in the year-classes, it is clear that we require observations on the pelagic stages — feeding habits, abundance of food, observed drift, hydrographical conditions. It will be remembered that Hjort[5]) was of the opinion that fluctuations may be determined at the stage when the larva has just absorbed its yolk-sac and is for the first time in need of external food, so that synchrony between this phase in pelagic life and the vernal outbreak of plankton might yield good brood years, and asynchrony bad years. More recently Sund has suggested a different cause, namely the amount of winter precipitation of snow, acting, perhaps, through displacement of the coastal water by melted snow-water, and the diversion of the stream of fry away from suitable nurseries[6]). (Thompson, however,

[1]) Strubberg, A. C. Marking Experiments with Cod (*Gadus callarias* L.) in Danish Waters. Medd. Komm. Havundersøgelser. Fisk. VII. 1. Copenhagen, 1922.

[2]) Jensen (1929) *loc. cit.*

[3]) Hessle, C. Undersökningar rörande torsken (*Gadus callarias* L.) i Mellersta Östersjön och Bottenhavet. Medd. Kungl. Lantbruksstyrelsen. No. 243. p. 19. Stockholm, 1923.

[4]) Wollaston, H. J. Buchanan. Plaice egg Production in 1920—1921, Treated as a Statistical Problem. Fish. Invest. II. IX. 2. London, 1926.

[5]) Hjort (1914) *loc. cit.* p. 205.

[6]) Sund, O. Snow and the Survival of Cod Fry. Nature CXIII. p. 163. London, 1924.

regards HJORT's explanation as the most probable for North Sea haddock[1]). ROLLEFSEN, last year, advanced a new theory, as to a contributory cause[2]), namely that eggs may fail to develope normally owing to disturbance by wave action at a critical phase. With regard to ROLLEFSEN's observations, however, it is almost certain that eggs must be very greatly more disturbed by the operations of washing the tow-net, and transferring the catch to vessels, than they ever can be when cushioned in water. Furthermore, ROLLEFSEN's statement that most of the cod eggs are usually in the surface layers of water, which he gives as a matter of common knowledge, is not borne out by experience in other regions[3]); and, finally, as ROLLEFSEN properly points out, the correlation observed between south-westerly, disturbing, winds, and bad brood years, is equally well explained on SUND's theory. It may be pointed out, that the three theories considered place the critical period at a different stage in the life-history, so that some selection among the three theories may be made by attempting correlation of the numbers of eggs or fry at different stages with the subsequent yield of the fishery. According to ROLLEFSEN fluctuations would already be determined by the time the egg hatches; according to HJORT the number of fry without yolk-sacs should be the criterion of fluctuation; and, according to SUND the fluctuations are determined later, by the drift of the older pelagic stages.

Summary.

There is evidence that in the Norwegian (SUND) and in the North Sea (RUSSELL, confirmed herein) stocks of cod, fluctuations in the abundance of year-classes are already determined by the time the cod reaches marketable size. It has also long been suspected that the critical stage is between the egg-stage and the assumption of demersal life, a theory for which POULSEN promises evidence. There may, therefore, be two methods of collecting statistics of abundance of the year-classes: (a) by observations of the density of the youngest marketable fish, (b) from the density and history of the pelagic fry. There appear to be four reasons for ultimately adopting *b*, rather than *a* (p. 17). Method *a* is established for Norwegian waters and for the North Sea; and it is recommended that this method should also be attempted in other regions. At the same time, it is recommended that research be put in hand for standardisation of technique for method *b*.

[1]) THOMPSON, H. Fluctuations in the North Sea Haddock Stock. Cons. Int. Expl. de la Mer. Rapports LXV. p. 35. Copenhagen, 1930.

[2]) ROLLEFSEN, G. Observations on Cod Eggs. Cons. Int. Expl. de la Mer. Rapports LXV. p. 31. Copenhagen, 1930.

[3]) e.g. JENSEN (1929) *loc. cit.* p. 14. Mr. WOLLASTON tells me that in rough weather plaice eggs are well distributed at all depths (in the Southern North Sea); but that in calm weather they tend to accumulate at the surface.

DISCUSSION

Investigations of Fluctuations in the Cod Stock in Danish Waters.

By

Erik M. Poulsen.

The fishery investigations during the last thirty years have distinctly shown that, from a practical point of view, it is very essential for us to know, as early as possible, in what quantities a certain fish will be available for the fishery in the nearest years to come.

This knowledge is of importance not only for the fishermen and the fishing industry but also for the fishery authorities, as it is on the basis of this knowledge that the proper protective measures can be taken, and in future, when compelled to proceed to the artificial rearing of various sea fishes it will be necessary for us to know how large will be the numerical strength of the different year-classes in the immediately following years.

In this connection it was of the greatest interest that Joh. Hjort in 1913 advanced the theory that the numerical strength of a year-class is apparently determined at a very early larval stage, and continues in approximately the same relation to that of other year-classes throughout the life of the individuals.

In the Danish Seas we have in the last 12 years carried out investigations to elucidate the fluctuations in the cod stock. The main results of these investigations and the material on which they are based will be published in a paper now in the press. I should here like to draw attention to some points which are of special interest for the problems under discussion.

In Fig. 1 I have given the results of the Danish investigations as to the fluctuations in the cod stock in the Belt Sea. The completely drawn curve gives the mean number of larvae caught per half hour with the ring trawl in March-April in the years 1923—1929. The other curves give the mean number of the 0-gr., the I- and II-gr. caught in a fine-meshed otter trawl "the Tog" per half hour in the years 1919—1929. We see that the four curves, as far as the investigations go, coincide with each other. When the amount of larvae is great in one year then the 0-gr. of the same, the I-gr. of the following, and the II-gr. of the year after are abundantly represented and *vice versa*. I may here add,

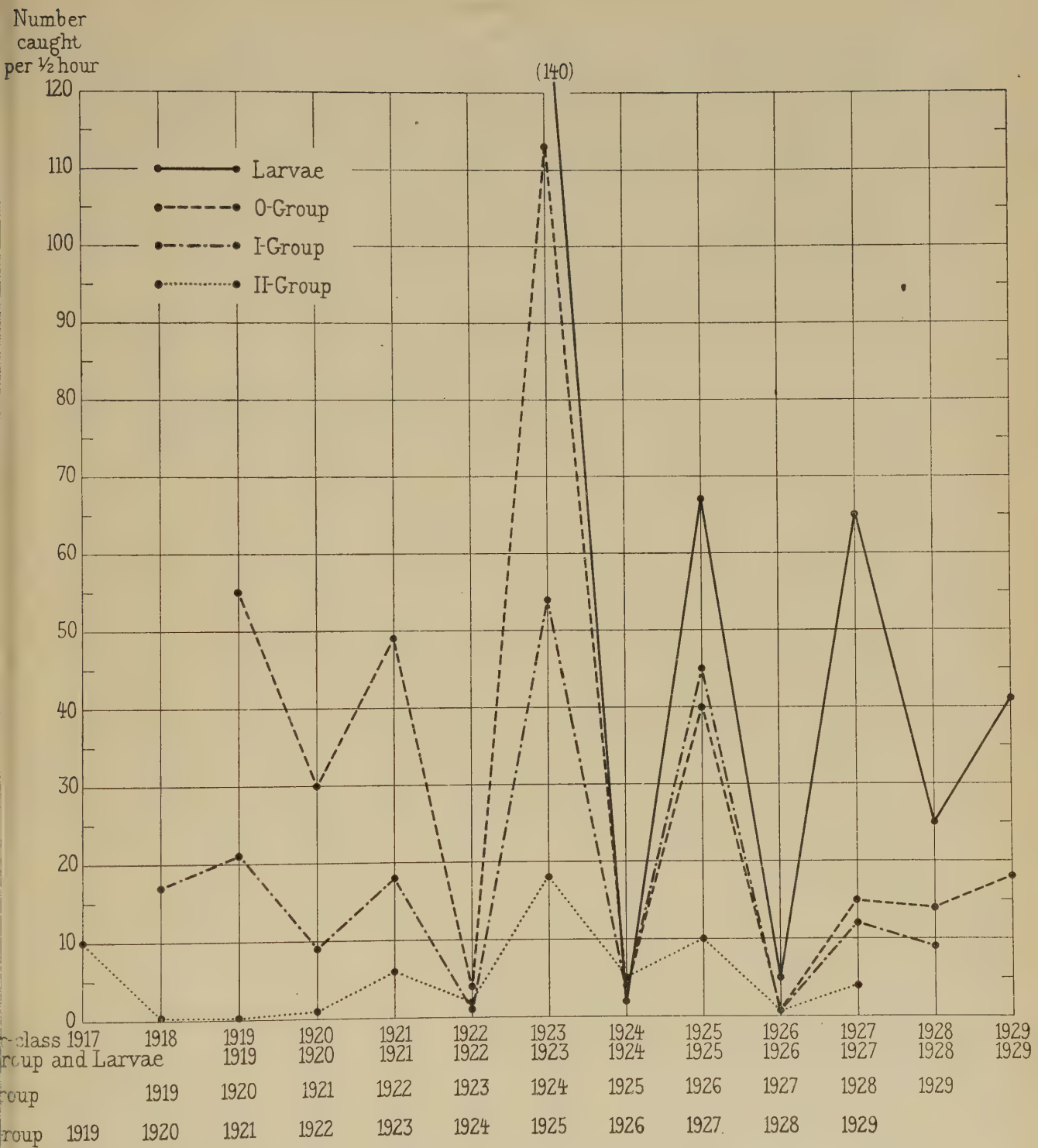

Fig. 1. Fluctuations in number of cod larvae in the years 1923—1929 and in number of individuals of the age-groups 0, I, and II in the years 1919—1929.

that in age-analyses of marketable fish from 1929 the same year classes (1925 and 1927) are found to dominate as in the analyses of the cod caught with the fine-meshed trawl or as larvae in the ring trawl. Of the year-classes older than 1925 only a very few individuals were present in the samples of marketable cod caught in the Belt Sea in 1929.

The figures here given show clearly that the larval stage is the critical stage in the cod's life, as it is at this stage that the numerical value of the year-classes is determined.

But the fact that a congruity, like that mentioned here, can be shown for as long a period as is the case, also tells us something that may be of interest for the discussion of the Validity of the Methods in Use, viz. that the results of the pelagic fishery for the larvae may be tested by bottom fishery for the youngest year-classes and further that in the present case the amount of larvae can be duly determined by a fishery with ring trawl carried out on such a scale as has been the case in the inner Danish waters in the years 1923—1930, and that the fishery with the small-mesh trawl gives us a truly representative picture of the numerical relation of the youngest year-classes. I am of opinion that the observations I here have put forward show that for the inner Danish waters we are able to procure, from year to year, material elucidating the relative numerical value of the different year classes.

In a paper in the report "On the Fluctuations in the Abundance of the various Year classes of Food Fishes" I have shown that there is a strong coincidence between the number of larvae in the months April-May and the temperature of the bottom water in March-May and the salinity in Nov.-Dec. high temperatures and high salinity coinciding with large quantities of larvae and *vice versa*. At the Scientific Meeting in London last year only material for the years 1923—1928 was to hand, now the material for the year 1929 is available and for this year the same coincidence is present. Also for the years before 1923 when we have no observations of the numbers of larvae but only of the quantities of the youngest year-classes there is a good coincidence between the strength of the year-classes and the hydrographical conditions. This can be seen from the following figures giving the average salinity of the bottom water at Schultz's Grund at the northern entrance to the Belt Sea during November and December throughout the period 1916—1928 together with the number of larvae caught per $^1/_2$ hour in the Belt Sea in 1923—1929. The signs + or ÷ denote whether the year-classes 1917—1922 were abundant or poor:

Year	1917	1918	1919	1920	1921	1922	1923	1924	1925	1926	1927	1928	1929
Abundance of year-classes, Belt Sea	+	÷	+	÷	+	÷	140	2	67	5	63	25	41
Salinity per mille in Nov. & Dec. of previous year	30.8	26.8	31.5	29.1	30.8	28.0	29.7	26.1	32.2	28.6	30.5	27.0	29.8

The congruity between the salinity in Nov.-Dec. and the strength of the year-classes is very striking.

The coincidence here shown promises us that we shall, as early as in December, be able to tell if the year-class that will be born in the following spring will be of more or less importance for the fishery.

In order to arrive at as close an understanding as possible of the fluctuations in the cod stock, it would be expedient to carry out investigations with the ring trawl in different places throughout the area of the cod's distribution in the months when the larvae occur most abundantly. As we have a fairly good knowledge of the distribution and occurrence of the larvae this investigations will, with the means now at our disposal for fishery investigations, by no means be impracticable. This ring trawl fishery ought, of course, to go hand in hand with investigations of temperature, salinity and quantity of plankton. A valuable supplement to this investigation will be weekly or fortnightly vertical hauls with the Hensen Net and fine plankton net from suitable light ships such as have recently been established by Dr. A. C. Johansen and Professor C. H. Ostenfeld for certain Danish lightships.

In the areas bordering the cod's distribution, racial analyses of the different age groups ought to be carried out in order to show how important the immigration from these areas is for the maintenance of the cod stock.

The closest possible co-operation of the various countries interested in the cod fishery and a yearly working together of the results is of course an indispensable basis for a gratifying result of the work.

II. HADDOCK

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On the Possibility of effecting and utilizing accurate Estimates of Haddock Fluctuations.

By

Harold Thompson.

A. Introductory Note.

One of the first questions to be considered when we contemplate the making public of the applied results of our biological studies on fishes is whether our preliminary serial observations have been continued on a consistent basis over a period fully expressive of the average fish life. It is a scientific truism that the longer one works at a problem the more of its intricacies become apparent and that the call for more and still more data for the elucidation of details gains in insistency. In things vital the unexpected can so often happen that only by virtue of experience can the danger be minimised of drawing wrong conclusions or effecting false prognostications from even the best of evidence. On the other hand, during our search after the scope of, and the factors controlling, the great fluctuations now proved to occur in the relative age-composition and the numerical density of the stocks of the important food fishes, it is well periodically to halt and survey methods and progress. In particular it is desirable to be reassured if possible as to the reliability of data from which deductions are being made, for it is in the soundness of the data rather than in the methods of manipulating the latter that security lies. This résumé of the stage reached in the studies on the fluctuations in the composition of the North Sea Haddock stock may therefore best be prefaced by an outline of the sources of the data worked upon, and tests may thereafter be made of the sufficiency or otherwise of these data for the purpose of utilitarian application. The North Sea Haddock investigations, in the form they now take, may be said to have been entered upon in 1922, so that in a year's time from now (i.e., in 1931) they will have been in progress for ten years. It is proposed to make a comprehensive review of the course of the work, and the results reached, in that decennial period. This résumé is in a sense anticipatory of part of that review and is given with the reservation that modification of detail may require to be effected in preparing the latter. This may well be since a considerable body of data bearing on the subject matter of the present paper has been accumulated but not yet worked upon. This extra material should for example assist in broadening our present knowledge of the chief Haddock migrations which must be taken account of when the question of the more local yield of the fisheries is being considered. Sufficient work has, however, probably been done on the main body of the data to show that there has been developed a practicable method of procedure which, while not involving a gigantic effort, yet serves consistently to survey the major changes in the North Sea

Haddock population, and so records these changes that the essential facts may take their place in a historical series available to the future worker, whatever changes of technique he may introduce. The method has already been outlined (*1*)[1]) along with the evidence supporting its reliability. It is therefore assumed that it has been proved that we are in possession of two all-important means for the study of fluctuations, viz. a means (the fish scale chiefly) of determinating the age of the individual fish, and one (the otter-trawl) of taking representative samples of the fish stock. Attention will be devoted here chiefly to the consideration of the question whether we are carrying on our sampling on a scale sufficiently large to furnish evidence upon which may be established a biological service of value to the fisherman (of value chiefly in that it will accurately forecast the latter's probable catch, and direct him to the areas where he may most lucratively work — without, it may be added, prejudice to his future welfare through undue exploitation of the stock).

B. The Existing Sources of the Data.

a) From Research Vessel Operations, and from Measurements effected on Commercial Vessels.

From these sources the most useful and indispensable data are to be obtained. The research vessel can carry through cruises organised to effect sampling in the most important portions of the area. It must be equipped with the standard apparatus in current use on commercial fishing vessels and, should the apparatus be modified or displaced by another, should, in order that conversion factors be obtained for the treatment of previously collected data, determine the relative fishing efficiency of new and old types of gear. As far as may be judged from the result of our attempt to follow the recent numerical changes in the North Sea Haddock stock, it seems probable that one such vessel, only part of whose time is devoted to the task, may in the course of each year effect sufficient observations for the approximate evaluation of these numerical changes. Fig. 1 shows the average number of observations made per annum (March 1922—March 1930) in each of the various areas into which the North Sea has been divided for the purpose of recognising distinctions in Haddock growth-rates. It will be observed that the most frequent trawling experiments have been carried out in the western and northern portions of the area where, fortunately for our purpose, young Haddock are most plentiful. Fewer observations have been made in the eastern and southern portions, and it is unlikely that, in consideration of the other claims on her services, the Scottish research vessel can there effectively increase her operations. But since the estimation of the mean density of the North Sea Haddock stock is and must be based on a method which places equal weight on the density in each unit of area (the small rectangles in Fig. 1) it is obviously desirable that more frequent observations should be made in the south and east. As a matter of fact fairly frequent observations are made by Fish Measurers accompanying commercial vessels on trips to various portions of these areas, but, unfortunately, it is impossible at present to make full use of their results. This is partly due to the fact that these results cannot include represen-

[1]) Italic figures in brackets refer to literature list p. 53.

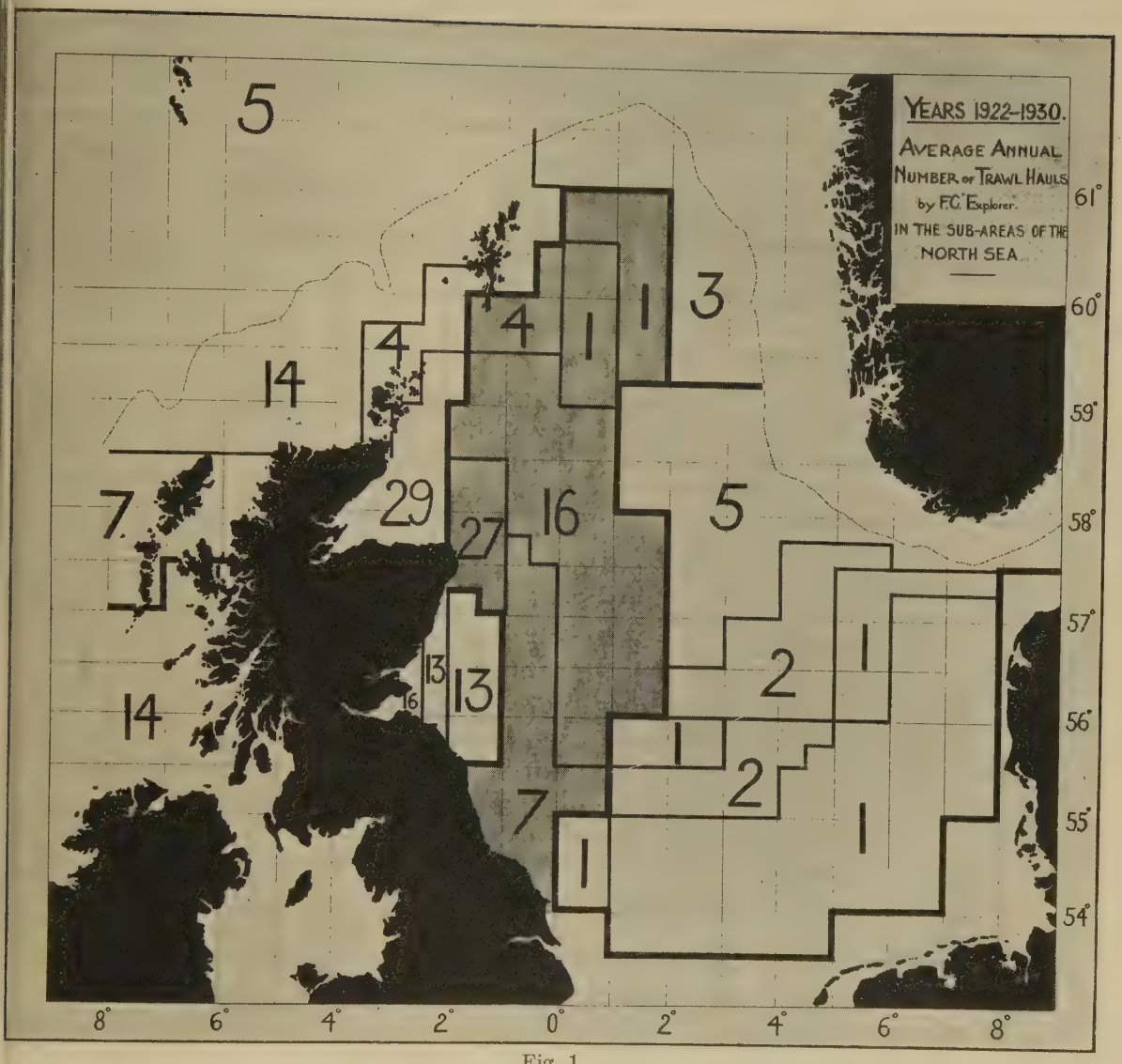


Fig. 1.

tative data as to the numerical density of the small first and second year Haddock, and such data are essential for our purpose. It is also due to the fact that, in the rather general absence of the means of determining the ages of the fish measured, there is usually no means of finding the proportional numbers of the older age-groups represented in the statistics of average catch in unit time of fishing. For example, Table A reproduces the statistics for certain areas in December of each of the years 1924 and 1925. (The returns for certain rectangles in *Area E* — that of fastest growth — and *Area c* — that of slowest growth — supply material for comparison.) It will be seen that the catches of 0-group Haddock (those up to ca. 19 cm. in length) are inconsistent. Thus the 0-groups

of years 1924 and 1925 respectively were, according to these returns, quite absent in *Area E*, while in *Area c* they were inconsistently sampled (cf. e.g., the two entries for each of the rectangles E11 and E12). Even for *Area c* any positive figures which occur for the 0-group are of little absolute significance owing to the fact that more Haddock of these sizes have escaped through the trawl meshes than have been retained by them. These records indicate, then, whether or not new broods are relatively numerous, but it is obvious that more specific information about such broods must be obtained by research vessels using a fine mesh covering to the ordinary trawl cod-end in order to prevent escape of the smaller fish. (Alternatively, some form of working agreement would require to be come to with the owners and skippers of one or two trawlers to the effect that, in return for fair compensation, they should, in each new area they visit, preface their ordinary fishing operations by one or two one-hour hauls with a spare trawl so covered. The contents of these hauls might be worked up by accompanying Fish Measurers, and material for complete age-assessment collected. In this way not only the preliminary but the subsequent hauls made would provide statistical evidence of the first class. This alternative proposition borders on the ideal, but will quite possibly prove incapable of being put into practice). Such information as may at present be gleaned from such statistics as are depicted in Table A depends largely on circumstances. Thus in *Area E* it is possible to make an approximate evaluation of the relative abundance of the 1923 brood during its second and third years. This is so since in this area growth is fast enough to prevent much overlap between the size-distribution of one year-class and its younger and older neighbours, and since, in this particular case, the older age-classes — those of 1921 and 1922 — were of negligible numerical proportions. In *Area c*, however, growth is so slow that overlap renders impossible even the rough separation of the various age-class size-distributions. Here especially it is essential that scales be taken for age-assessment. The fact is that if accurate age-assessment could be made of these widely-collected statistics the information to be derived therefrom would be invaluable, instead of being very fragmentary as it is at present. But it must be remembered that the commercial trawl haul is usually of several hours' duration and that inevitably most of the fish's scales are during this time chafed off. Such scales as remain are not usually of the type which we desire since they must be taken from more protected portions of the fish's body (under fins etc.). Apart then from the possibility of research vessels taking trawl-samples in the same areas as those fished in by the commercial vessels which are accompanied by Fish Measurers, and so obtaining key material, it seems that, as far as the Haddock is concerned, the only means of fully utilising these important commercial statistics is that suggested above. The adoption of this procedure is of course open to any country materially interested in the Haddock Fisheries, whether or not that country has a research vessel in regular commission. Whereas almost entire reliance has had to be placed in the past on the necessarily restricted sampling efforts of research vessels, it is probable that we should look rather to obtain from these vessels only supplementary data, gathered with a specific object in view (such e.g., as endeavouring to trace the causes of fluctuations or to elucidating unforeseen difficulties which may from time to time arise in carrying out the general survey). It is obviously unwise,

Table X. Showing the influence of *Calendar Year* (Columns I and II; III and IV) and of *Area* (Columns I, II—III, IV) on composition of Catch.

Sizes of Haddock in centimetres; Catches per 10 hours' fishing; * Vigneron-Dahl Gear.

Area **E** (of fast growth)

Square	I. Numbers caught Dec. 1924 F7	G6	G7	H6	Totals		II. Numbers caught Dec. 1925 G7	H6	H7	H7	J7	J7	J8	Totals	
Size 14	Mean Sizes of Broods in						..	..	*	..	*	..	*	..	
15	December over the whole						..	..	..	..	..	..	..	..	
16	of *Area* **E**:—						..	..	..	..	..	..	..	..	
17	Second Year 29.0 cms.						..	..	..	..	..	..	..	..	
18	Third — 35.5 —						..	..	..	..	..	..	..	..	
19	Fourth — 40.2 —						..	..	..	..	..	..	..	..	
20	Fifth — 44.6 —						..	..	..	..	..	..	..	..	
21	3	..	..	..	3		..	..	..	..	..	..	..	..	
22	..	3	..	..	3		..	..	..	..	..	..	..	..	
23	3	1	..	..	4		..	..	..	..	..	..	..	..	
24	16	4	1	3	24		..	..	..	..	..	..	..	..	
25	51	12	10	7	80		..	..	..	..	..	..	..	..	
26	119	23	29	10	181	1923 Brood (Second year)	5	..	..	..	9	..	..	14	1924 Brood (Third year)
27	174	77	59	40	350		14	..	..	11	16	8	13	62	
28	394	179	75	53	701		25	11	93	26	67	27	13	262	
29	452	277	143	140	1012		81	60	467	86	154	23	35	906	
30	741	365	147	173	*1426*		124	49	327	169	270	45	96	*1080*	
31	609	408	124	153	1294		152	49	280	217	216	52	72	*1038*	
32	463	320	122	160	1065		147	100	280	157	207	50	48	9894	
33	312	193	61	120	686		143	60	233	123	148	75	24	826	
34	168	103	48	43	362		101	31	93	157	157	61	37	637	
35	81	43	23	30	177		138	20	93	146	251	89	96	833	
36	4	15	5	..	24	1922	224	..	420	217	272	70	59	1262	
37	9	5	1	10	25	Brood	265	29	233	97	361	150	72	*1207*	
38	4	4	..	..	8		278	69	420	134	377	137	83	1498	
39	4	2	1	7	14		245	40	233	280	218	129	96	1241	
40	6	..	2	20	28	1921	216	40	187	134	235	79	83	974	
41	..	3	..	7	10	Brood	205	60	93	97	165	67	48	735	
42	7	4	11	..	22		159	60	233	134	107	4	13	710	
43	..	7	..	7	14		76	29	47	86	45	12	24	319	
44	7	4	4	7	22		43	11	47	74	48	20	..	2443	
45	4	3	5	7	19	1920	58	11	47	12	18	11	..	157	
46	..	7	5	..	12	Brood	19	11	47	49	23	4	..	153	
47	4	3	4	7	18		20	..	..	11	23	9	24	87	
48	4	4	1	20	29		14	..	..	..	2	..	13	29	
49	3	4	5	10	22		22	..	..	37	13	8	..	80	
50	4	4	5	20	33		7	..	..	..	..	..	..	7	
51	12	2	11	10	35		10	..	47	..	..	10	..	67	
52	..	3	5	7	15		6	..	..	..	..	..	..	6	
53	..	2	4	3	9		..	..	..	..	2	..	..	2	
54	12	1	1	3	17		5	..	..	..	..	..	..	5	
55	..	2	1	..	3		6	..	..	..	..	..	..	6	
Over 55	16	2	7	3	28		11	..	..	..	..	..	..	11	

Area **c** (of slow growth)

III. Numbers caught Dec. 1924 E10	E11	E11	E12	E12	Totals		IV. Numbers caught Dec. 1925 E10	E11	E12	Totals	
..	4	*	7	*	11		*	*	*	..	
..	15	6	15	..	36		..	..	..	..	
..	30	6	17	..	53		..	..	..	..	
..	52	..	16	..	*68*	1924	..	..	..	..	
..	38	..	1	..	39	Brood	..	..	2	2	
..	47	65	27	70	209		..	2	4	6	
65	109	286	140	301	901		7	11	18	36	
265	443	975	418	1019	3120	1923	2	79	66	147	
621	808	1360	538	1530	*4857*	Brood	28	206	169	403	1924
792	679	548	308	772	3099	(2nd	88	463	386	937	Brood
623	399	151	133	221	1527	Year)	127	944	831	1902	
229	131	18	38	38	454		242	1230	764	*2236*	1923
73	74	52	20	107	326		342	1197	850	*2389*	Brood
77	76	65	23	232	*473*		329	782	564	1675	(3rd
73	66	28	36	159	362		312	399	262	973	Year)
132	102	151	58	263	*706*		273	360	275	908	
51	102	28	41	122	344		150	216	141	507	
85	81	52	31	104	*353*		83	182	141	406	
69	113	46	36	92	356		65	135	146	346	
29	48	28	12	35	152		31	73	61	165	
36	40	..	14	17	107		23	62	38	123	
11	10	6	6	8	41		19	37	17	73	
10	20	..	2	9	41		17	22	25	64	
12	5	..	2	..	19		9	7	8	24	
..	5	..	1	..	6		5	4	13	22	
3	3	..	1	..	7		5	2	6	13	
3	..	..	1	1	5		13	1	..	14	
..	4	..	1	6	11		6	1	3	10	
..	5	6	..	..	11		3	..	..	3	
..	4	..	..	..	4		8	..	..	8	
..	1	..	..	..	1		4	..	..	4	
..	..	..	..	..	..		3	..	..	3	
..	..	..	..	..	..		..	..	..	..	
..	..	..	..	..	..		Mean Sizes of Broods in December over the whole of *Area* **c**:—				
..	..	..	..	..	..		First Year: 17.0 cms.				
..	..	..	..	..	..		Second — 23.4 —				
..	..	..	..	4	4		Third — 26.9 —				
..	..	..	..	..	..		Fourth — 29.2 —				
..	..	..	..	..	..		Fifth — 30.3 —				
..	..	..	..	..	..		Sixth — 33.0 —				

Column I | Column II | Column III | Column IV

The location of the squares mentioned in this table may be determined by reference to Fig. 1.

Modal sizes of well-represented broods given in italics.

if we are considering the continuous maintenance of a public service, to be forced to rely for our chief data upon the activities of one or two research vessels upon whose time other demands are liable to occur. If, however, this is the only feasible course to be followed it is essential that, in the interest of the studies of the more valuable demersal food-fishes, all the important ground be covered by a regular research vessel trawling survey.

b) Statistics of Weights of Fish landed by British Commercial Vessels.

Since the year 1923 there has been compiled (by the statistical staff of the Ministry of Agriculture and Fisheries, England) a further set of commercial statistics referring to the weight of Haddock caught by British vessels. These statistics give the gross weight of Haddock caught, and the average catch in unit time of fishing, in each area represented by the small rectangles of Fig. 2, which is virtually a facsimile of the return for the year 1928. Similar returns are, however, made for each month of the year, and show in a general way the shifts of the chief centres of the Haddock population. By summing the average catches of the various rectangles and dividing by the number of rectangles for which there is an entry, we obtain a figure which, since equal importance is assigned to each area, may be taken as fairly representative of the density of stock at the time in the North Sea generally. The average weight caught per month (or per year) is really equivalent to that which would be caught by a trawler working for a standard length of time in each of the rectangular areas of the North Sea, and the series of monthly averages provides in effect the best possible index at present available of the fluctuations occurring generally in the apparent density of the Haddock stock. That this index is indeed reliable seems probable if we test it against the brood estimates ascertained by the entirely different method of age-determination of catches made by the research vessel in unit time of fishing (Fig. 3). As has before been mentioned (2) the weight of Haddock actually caught by commercial vessels can be shown to be closely connected with the relative success or non-success of broods which have, at the time of reckoning, attained the age of from two to four years (due allowance being made of course for seasonal variation since the latter occurs independently of the density of the stock). This connection might be expected to be still closer were the two sets of data strictly comparable in the sense that they related to precisely the same superficial area. The research vessel results actually refer to the whole area (including the waters off the West Coast of Scotland) which might be regarded as self-contained as far as the Haddock stock is concerned; the commercial statistics refer to the major portion of this area and it is hoped will in future be augmented by those for the West Scottish coast.

Both these sources of data yield information only with regard to the Haddock bottom stages — that is with regard to the major portion of the life of the broods from the age of from about three to six months onwards. One cannot conceive of an organised study which could estimate the degrees of success of the various broods by the determination of the relative numbers of eggs, larvae or fry in the plankton in the various years. Apart from all consideration of the vast amount of labour which would be involved in examining a sufficient number of plankton collections to be at all representative, there

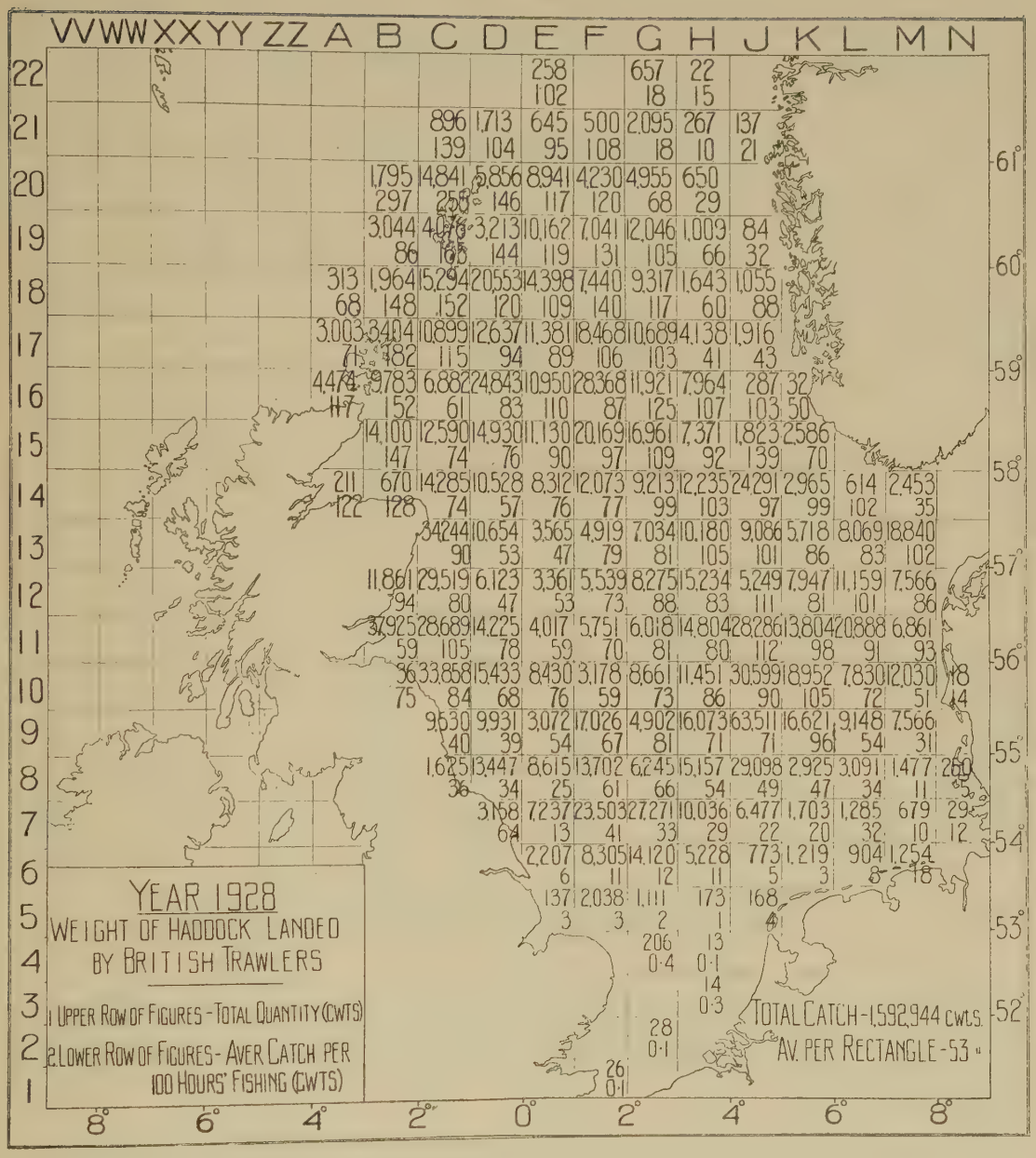


Fig. 2.

is little likelihood of such representative collections being made year after year in the few months during which the plankton stages are available. The point in the life history from which we may best commence to determine degree of brood survival is therefore largely determined for us by economic necessity, and, in the case of the Haddock, may be defined as the time when the demersal habit of life is assumed (only incompletely,

it is true) some few months after the breeding season. From this point — or from a later point if necessary — the series of estimations can proceed uninterruptedly with the one standard sampling instrument — the otter trawl whose cod-end is covered by shrimp-netting.

It is on this basis that the fluctuations in the degree of survival of recent North Sea Haddock broods have been determined. The extent of these fluctuations has already

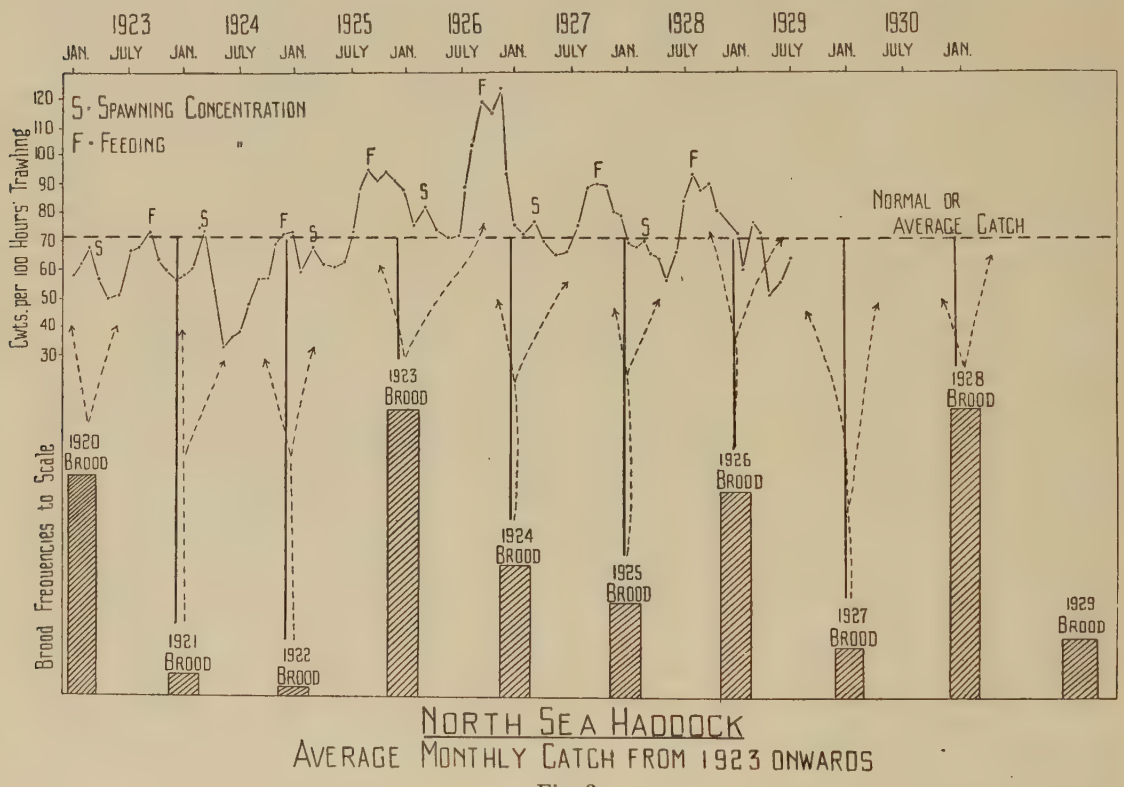


Fig. 3.

been referred to (*1*, *2*). Here we are concerned chiefly with the possibility of establishing a connection between the estimates formed (as promptly as possible) of the survival values of broods, and the catches which the commercial fleet will later on make of these broods.

C. Tests of the Efficiency of the Methods of Collecting and Treating the Data.

a) Comparison of actual Yield in recent Years with Yield which should theoretically have been available.

Having worked out the relative numerical density of the various age-groups of Haddock in each of the years 1923 to 1928 inclusive, we may, by multiplying each of the age-group frequencies by the appropriate weight factor, find the theoretical available weight of each brood, and the total weight of all broods combined, which might each

TABLE B.

NORTH SEA HADDOCK

I. Average Number of each Age-class caught (research vessel) per 100 hrs' fishing.
[Number theoretically available]

	AGE GROUP					
	(1) * Second.	(2) Third.	(3) Fourth.	(4) Fifth.	(5) Sixth.	(6) Seventh.
In 1923	330	1720	9090	1310	200	70
" 1924	28060	390	860	4100	820	150
" 1925	11920	23860	160	300	1490	590
" 1926	9640	11530	8510	130	300	640
" 1927	15240	6670	4640	3530	100	230
" 1928	2920	15990	2170	1250	590	30

* Only half the second year number was actually taken into reckoning.

II { a. Theoretical Catch by Weight calculated from I in cwts. per 100 hrs' fishing.
b. Actual Catch in cwts. per 100 hrs' fishing.
c. Total Catch in cwts. for each year

	YEAR.					
	(1) 1923	(2) 1924	(3) 1925	(4) 1926	(5) 1927	(6) 1928
a.	65	74	117	103	90	83
b.	62	57	73	88	77	73
c.	1,467,000	1,314,000	1,977,000	1,691,000	1,629,000	1,543,000

III as II, but with results expressed as percentage of highest catch for any one year.

	YEAR.					
	1923	1924	1925	1926	1927	1928.
a.	56 %	63%	100 %	88%	77%	71%
b.	70 %	65%	83%	100%	87%	83%
c.	74 %	66 %	100 %	85%	82%	80 %

year have been expected to be caught per average of 100 hours' fishing. The numbers of each brood caught in the various years are given in Table B, 1. As Haddock of over seven years of age are caught in very limited numbers, no account is here taken of fish in the eighth, ninth or tenth year of life. These frequencies were calculated for the natural

year (April to March), whereas the fishing results with which they are to be compared refer to the calendar year, so that the theoretical yield for any one year (say 1925) really refers to the closing nine months of that year and the first three months of the following year (1926 in this case). To this extent the comparison is not strictly allowable and should be limited to an examination of the tendency for both types of yield to fluctuate in the same general fashion.

In assigning the weight factors to the various year-classes we must first of all determine what will be the probable average sizes of the Haddock (of each age-class) caught in the course of the year. These sizes are of course unknown, but may be taken as rather greater than those reached half-way through the growth season (since the height of the feeding season, together with the breeding season, during which seasons most Haddock are caught, occur in the last seven months of the natural year). For the purpose of making our test calculations the average size caught during the year may be taken as being about 2 cms. less than that reached by North Sea Haddock (*I*, p. 146) at the end of the natural year — i.e., as being 23, $27^1/_2$, $31^1/_2$, 35, 39 and 41 centimetres for age-classes 2 to 7 respectively. Only $1^1/_2$ cms. have been deducted, however, in the case of second-year fish, since it is not until a size of 22 cms. is attained that the great bulk of Haddock are retained by the trawl meshes, so that to deduct 2 cms. from the end-year size ($24^1/_2$ cms.) and leave the average-size caught as $22^1/_2$ cms. would probably be to effect an under-estimation.

If we regard these fish lengths as approximating closely enough for our purpose to the actual average lengths of the Haddock of each age-class captured during the course of a year, it is next necessary to find for each of the sizes quoted an average weight-factor. Russell (*3*) has carried out extensive weighings of Haddock in the cleaned condition — that is in the condition in which they are landed and taken account of in the commercial statistics we are to bring into comparison with our theoretical results. He has tabulated the weights of the fish of various sizes according to month of capture, since the weight of a fish of a certain size may rise or fall according to the condition of the fish. Taking the weights for the month of August (mid-way through the feeding season) as being typical, we find that Haddock of the sizes quoted above weigh respectively 105, 170, 259, 364, 509 and 595 grams. Effecting the product of these figures with the appropriate frequency figures in Table B, I, and summing the products for the various age-classes in any one year, we obtain the theoretical yields in unit time of fishing (Table B, IIa). In the respective entries opposite *b* and *c* in this portion of the Table are found the actual commercial catches in unit time and the total British catches for the year. It will be seen that, on our assumptions (made, it is to be understood, merely for the purpose of our test) — (1) that the average size of the fish caught throughout the year is 2 cms. less than that generally reached at the end of the year and (2) that roughly only about one-half of the second year fish entering the trawl are, on the average, retained by it, or are brought to market[1]) — our calculations of the weight-yield in the

[1]) This assumption, or one of similar degree, is of course essential and legitimate, since a large number of second-year Haddock actually escape through the meshes, or, if caught, are rejected.

various years give results of very much the same order as those actually achieved. This is a finding of first rate importance, since it indicates that our methods of sampling are on a scale which probably only requires some adjustment and amplification in order to give results which will be sufficiently accurate for the purpose of anticipating the general yield of the North Sea Haddock fishery for at least one year ahead. The theoretical results are, however, all on the high side (about 24 per cent. on the average) and it is noticeable that, in general, the greater the number of young and therefore small fish of the second and third year, the greater is the excess of the theoretical over the actual yield. This suggests that our trial allowance (one-half of the numbers of second-year Haddock entering the net) for "escapes" and "discards" is, in certain circumstances at least, rather on the low side, and will require modification.

Although it is evidently to be in our power, if we have pre-determined the brood-frequencies in the early part of a year, to estimate the yield to be obtained in unit time of fishing in that year, we are bound to take notice of the fact that the maximum average catch (theoretically) would have occurred in 1925, while the maximum catch did actually occur in 1926. The comparison is clearer (Fig. 4) if we render the results, as in Table B, III, as percentage amounts of the maximum catch (i.e., of 117 and of 88 cwts. respectively in either set of computations). At the same time the opportunity may be taken of bringing into the comparison the total weights (also as percentage amounts of the highest catch — that of 1925 in this case) of North Sea Haddock landed in Great Britain. Of course, since these total weights do not take into account the number of hours spent in trawling, they are only roughly indicative of any considerable increase or decrease in the density of the Haddock stock. There is general agreement in the course taken by the three graphs, but the one representing the theoretical yield disagrees with that (with which it is most directly to be compared) for actual yield in unit time in that the greatest catch is shown as due to occur in 1925, not 1926; it therefore agrees in this respect with the graph for the total catch. Thus the estimation of the theoretical yield in unit time disagrees, for the years 1925 and 1926, with the actual yield, but is in conformity with an actually greater total catch achieved in 1925 than in 1926. By adopting the procedure of smoothing in threes all three sets of percentage figures we can obliterate this point of difference and obtain concordance[1]) in the courses of the three graphs, but such a procedure is not available if we are to aim at a prospective instead of, as in this case, a retrospective set of estimations. We must therefore seek for and endeavour to eliminate such faults in our data as are likely to lead to such disagreement of results as that mentioned above. One source of discrepancy lies in the fact that the theoretical (but not the actual) results are calculated for the natural fish-year; now if 1926 were indeed the peak year for catches, the natural year 1925 would "borrow" on the first three months of that year, and so tend to have its estimate improved upon relatively to that for the natural year 1926, which would have its estimate decreased since the first three months

[1]) Such concordance is indeed obtained if, instead of calculating from the brood frequencies of the current year, we do so from the modified frequencies of the previous year (see Fig. 6).

of the more meagre year 1927 would be taken into reckoning. To avoid this overlap between the natural and the calendar years it is essential that the commercial statistics should be compiled for the natural, in place of the calendar, year. One other explanation

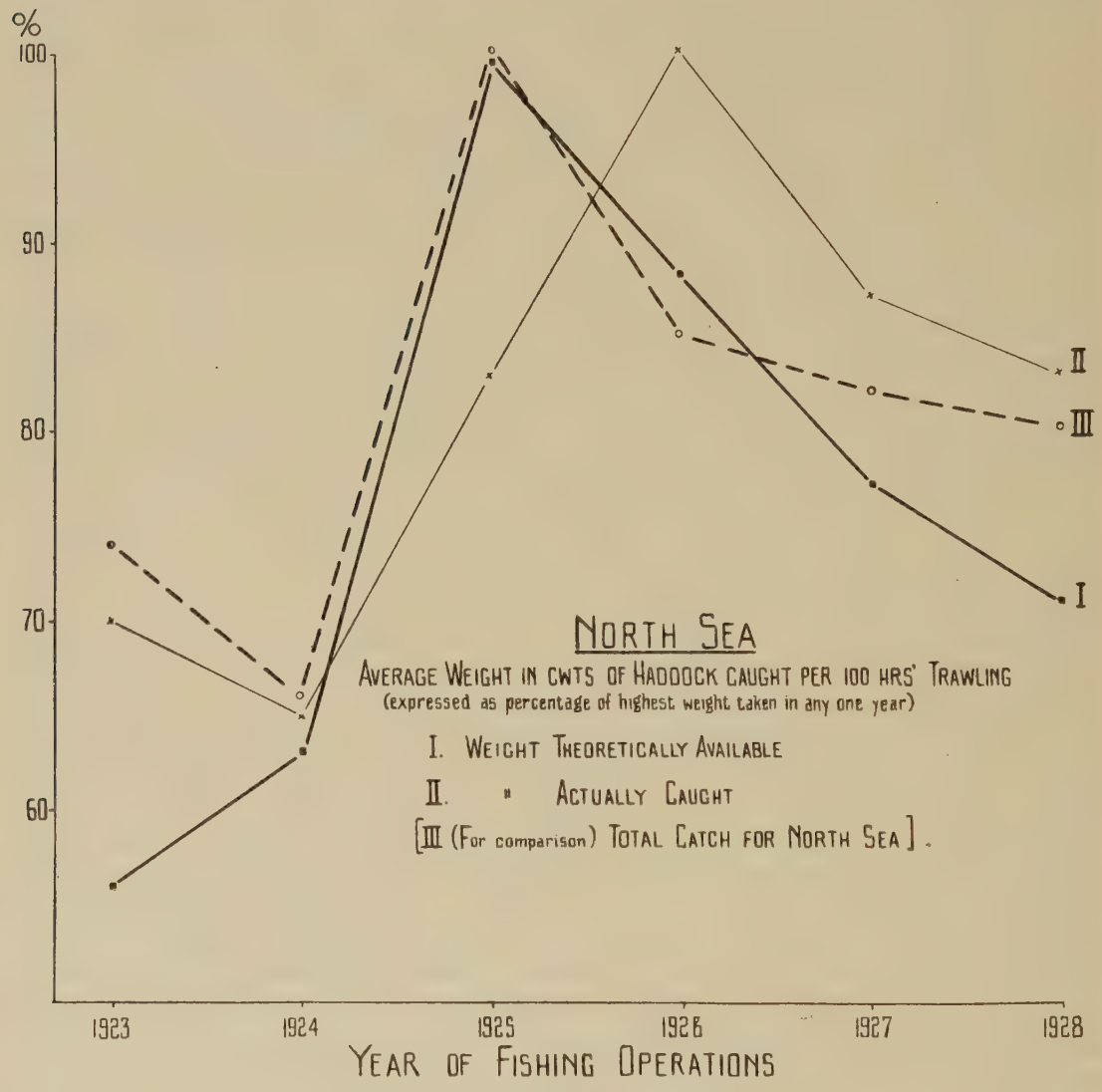


Fig. 4.

of at least part of the disagreement suggests itself. The total commercial returns for 1926 would tend to be low on account of the decreased fishing activity resulting from the general strike which occurred in the spring months of 1926. Owing to the same cause the Haddock stock was afforded a certain immunity from diminution during the period that the plentiful 1923 brood, which was unusually well distributed in the south, was

putting on part of its fourth year's length increment. Later in the year, when trawling was fully resumed, rather more and larger fish than would otherwise have been available would be caught. If, however, as seems likely, 1926 was actually the peak year for catches, it is apparent that the sampling carried out by the research vessel was insufficiently extensive to discount the effect of the overlap between the research vessel- and the statistical-year, or that of any abnormal event, such as the strike, which might tend to disturb the balance between the powers of weight-recuperation, innate in the Haddock stock, and weight-elimination by the fishing fleet. At all events it should be possible to arrange that generally fuller data (especially data relating to the transitory feeding shoals in the southern and eastern portions of the North Sea, where the 1923 brood was unusually plentiful) be made available, for if we are to undertake the task of making forecasts it is essential that our minds be relieved of any perplexity as to the representative nature of what is in effect our prior information.

b) Comparison of actual Yield with Yield estimated from Frequency of Age-Groups in the previous Year.

In the above discussion note has been taken of the possible effect (in 1926) of an alteration in the rate of elimination of the age-classes of Haddock — as far as this rate is due to man's activities — on the subsequent fishing results to be achieved. It is highly probable that the existing rate of reduction in the numbers of Haddock broods is largely accounted for by the inroads made by intensive commercial fishing. Thus when this fishing intensity is relaxed (as it was during the war) there is a most noticeable increase in the average age of the stock, an increase indeed which tends to make the proportional numbers of the various age-classes more like those existing even at the present day on grounds like Rockall Bank and the White Sea, which, although attracting an increasing amount of attention, are not yet subjected to such intense fishing operations as take place in the North Sea. Mortality due to natural causes is therefore probably of relatively insignificant account. Trawlers alone remove some 4000 cwts. (about a million of Haddock) *per diem* from the North Sea, and it seems to be at least highly improbable that the natural mortality rate could be of this order. In computing the mortality-rate of North Sea Haddock therefore we are probably committing but a slight error if we ignore any possible changes in the natural rate of mortality, and seek to connect changes in the rate with changes in the methods or the intensity of fishing. Now the possibility of the alteration in the rate of stock depletion from time to time must be borne in mind in devising methods of forecasting fishing results. For example it will be shown below that our data seem to indicate that in recent years there has been a tendency for the rate of depletion of North Sea Haddock broods to be slightly accelerated. Whether or not this implication of the data is correct it is not difficult to understand how such a state of affairs could have come about. The existing need is to capture more fish (for the same expenditure of time) than were caught in the days of lower overhead costs. The need has probably accelerated the creation and the adoption of the

means (an improved trawl) of doing this, for, apart from the question of costs, there has been, since the war, a transition from an accumulated stock of larger Haddock to one quickly depleted of the latter and, further, poorly reinforced by brood fish in two successive years (1921 and 1922). Fortunately the tandem occurrence of two such "poor" broods is infrequent, but it served in this instance to lower the catches of the old type of trawl to meagre proportions in 1923 and 1924, just at the time when the advantages of the more efficient V.D. gear were being advertised to the trawlerman. The V.D. trawl has rapidly come into universal use, with the undoubted result that more Haddock have been caught in unit time than hitherto (without being compensated for by the effecting of a corresponding diminution in the time spent in fishing operations)[1]). The result has been that an increased toll has been taken of North Sea Haddock with the effect that the relative numbers of large fish have shrunk still further, so that in order to maintain the total landings at or near the pre-existing level the fisherman now brings in a larger proportion of the small Haddock caught. Ways and means of disposing of these small fish are being developed and if successful may lead to an increased demand for small Haddock, the satisfaction of which would entail increased and directed inroads being made on the young shoals. It is impossible now, and it will probably be impossible at any time in the future, to say that a definite mortality rate exists or is likely to persist, since the equilibrium between the recuperative power of the stock and the yield taken from the stock is ever altering. The alteration is due partly to the great natural fluctuations in the age-composition of the stock, which control the supply; and partly to the efforts of man to smooth over the effects of these fluctuations and cater for a steady or increasing demand. It is obvious then that the mortality rate will require continuous determination as an essential factor in any formula devised for forecasting future yield. Lea (4) has worked out, in the clearest possible manner, a formula expressive of the mortality rate of the Tribe of Norwegian Herring, which presents a rather specialised case and one particularly amenable to such treatment as he has devised. In the case of the North Sea Haddock, however, there is available for review a much shorter period during which quantitative observations (on an age-class basis) have been in progress. Even within that short period, as will be shown, the mortality rate has probably altered. On these considerations no attempt will be made here to derive a formula expressive of the general rate of mortality. At the same time a study of the facts will probably serve to indicate the probable rate of depletion which has been in action in recent years, and to provide a hypothetical basis for the determination of future yield.

[1]) The total number of hours' trawling in the North Sea was:—

In 1923 — 2,800,000	In 1926 — 2,200,000
1924 — 2,700,000	1927 — 2,600,000
1925 — 2,700,000	1928 — 2,600,000

The low total for 1926 may be taken as being due chiefly to suspension of trawling during the General Strike.

1. The apparent Rates of Depletion (Mortality Rates) of recent Haddock Broods in the North Sea.

The term "apparent Mortality Rate" is here used in a restricted sense in so far as it is derived from fishing results only, these results tending, in general, to make it apparent to the fisherman as time goes on that he captures fewer fish of any one brood. The actual rate may be greater or less than the apparent — less, for example, if Haddock as they become older migrate out of the area, become more nomadic, or partially forsake the completely demersal habit.

Table F (in *I*) which expressed the relative numerical proportions of recent Haddock broods during each year of life, has been brought up to date, and Table C, I, showing the percentage loss of numbers from year to year has been prepared from it. (In Table F, however, factors were used for raising the empirical values of the first year catches of those broods (mostly the less successful Broods) which appeared in the research vessel catches actually in greater numbers in the second than in the first year of life. For the preparation of Table C the empirical values were reverted to, so that several entries in column A show the positive sign, and even influence the average gain or loss result for all Broods sufficiently to indicate that, according to our methods of sampling, these Broods were more numerous in their second than in their first year of life). The truth is, of course, that since there must obviously be some diminution of numbers during the first year, our means of sampling very young Haddock shoals are insufficient, and especially so in the case of the less successful broods. What we really learn authentically is whether a new brood is relatively very abundant or otherwise. Fortunately, however, the disability to effect accurate estimates of the numbers of 0-group Haddock is chiefly restricted to the "poor" broods. In the case of broods which appear in great numbers the trawl rarely fails to effect fairly adequate sampling, even during the first year of life of these broods. It is broods of the latter type which will affect the future fishing returns, and of which we require the best possible early information. Good as our first estimates of the numerical density of these broods has been, it should be possible by the adoption of a wider range of sampling to improve on their accuracy.

It will be observed that the positive sign is found (elsewhere than in column A) only twice in Table C, and that in only one case (in column J) is this deviation from the normal serious. In the case of older fish, so attenuated in numbers, and resembling in this respect the young fish of a meagre brood, it is doubtful whether increased sampling would altogether eliminate the possibility of the positive sign appearing at any time in such a Table as this.

The results shown in the Table suggest that up to the third year of age (when on the average maturity is reached) the rate of mortality is comparatively low — certainly not greater than of the order of from 15 to 25 per cent. each year. Consistency among the figures is absent in column A, but begins to appear in column B, which shows the losses sustained between the second and third years of life. Some reliance may probably be placed upon the results shown in this column and still more so on those shown in column C, which are characterised by a high degree of consistency. A comparison of

TABLE C

I RECENT NORTH SEA HADDOCK BROODS: PERCENTAGE LOSS OR GAIN IN NUMBERS WITH INCREASING AGE

(Numbers reckoned from average numbers captured in unit time throughout each year of life)

	APPARENT LOSS OR GAIN IN PASSING FROM AGE :-								
BROOD	1 to 2 A	2 to 3 B	3 to 4 C	4 to 5 D	5 to 6 E	6 to 7 F	7 to 8 G	8 to 9 H	9 to 10 J
1913									- 85
1914								- 96	0
1915							- 85	- 50	- 67
1916				- 58	- 36	- 92	- 17	- 80	- 30
1917			- 64	- 59	- 50	- 75	- 29	- 32	- 88
1918		- 8	- 82	- 35	- 77	- 25	- 33	- 96	+ 125
1919	- 29	- 29	- 58	- 58	- 37	- 28	- 83	- 67	- 91
1920	- 14	- 23	- 46	- 55	- 64	- 57	- 52	- 87	- 85
1921	+ 46	- 20	- 50	- 65	0	- 23	- 91	- 90	
1922	+371	- 20	- 59	- 19	- 23	- 70	- 96		
1923	- 10	- 15	- 64	- 59	- 83	- 71			
1924	+ 66	- 3	- 60	- 73	- 61				
1925	+103	- 31	- 67	- 58					
1926	- 45	+ 5	- 71						
1927	- 20	- 9							
1928	± 0								
TOTAL	+ 468	- 153	- 621	- 539	- 431	- 441	- 486	- 598	- 321
AVERAGE	+ 47%*	- 15%	- 62%	- 54%	- 48%	- 55%	- 61%	- 75%	- 40%

← – – – – – – – AVR. -56% – – – – – – – – →

* For the six broods showing a loss the average loss is 20%.

II RESIDUAL NUMBER OF HADDOCK (♀) OF 1920, 1923 and 1926 BROODS PER 100 SURVIVING IN FIRST YEAR, ALLOWING 20% REDUCTION IN 1st and 2nd YEARS, AND THEREAFTER REDUCTION FOR EACH BROOD AS SHOWN IN I.

	RESIDUAL NUMBERS PER ORIGINAL 100 IN YEAR.					
BROOD	2	3	4	5	6	7
1920	80	64	35	16	6	2
1923	80	64	23	9	2	
1926	80	64	19			

III. AVERAGE ANNUAL PERCENTAGE LOSS OF NUMBERS (δ) FOR BROODS PRIOR TO AND SUBSEQUENT TO THE 1920 BROOD (but omitting the meagre broods 1921 and 1922).

BROODS	MEAN YEAR OF CATCH	AVERAGE LOSS IN PASSING FROM AGE :-						
		3 to 4	4 to 5	5 to 6	6 to 7	7 to 8	8 to 9	9 to 10
1913 - 1920	1922½	62 %	53 %	53 %	55 %	50 %	73 %	40 %
1923 - 1928	1928	66 %	63 %	72 %	71 %			

IV. COMPARISON OF NUMERICAL CATCH OF OLDER AGE CLASSES AT ROCKALL (MAY 1929) AND OF THE TYPICAL 1920 BROOD IN SUCCESSIVE YEARS IN THE NORTH SEA.

	CATCH PER 10 HOURS' FISHING OF AGE-CLASS –											
	4	5	6	7	8	9	10	11	12	13	14	TOTAL
NORTH SEA	909	410	149	64	31	4	0·6	0·1	0	0	0	1568
ROCKALL	1291	757	343	86	211	148	32	158	17	1	12	3056

these neighbouring columns shows that there is ostensibly a marked increase in the mortality rate (to about 62 per cent.), this increase first taking effect between the third and fourth years of life. This increased rate is more or less regularly sustained in later years — the average rate for ages of more than three years is 56 per cent. — so that, under present conditions, we will probably be committing but a small error if we use this percentage-figure as expressing the annual rate of declension in the numbers of

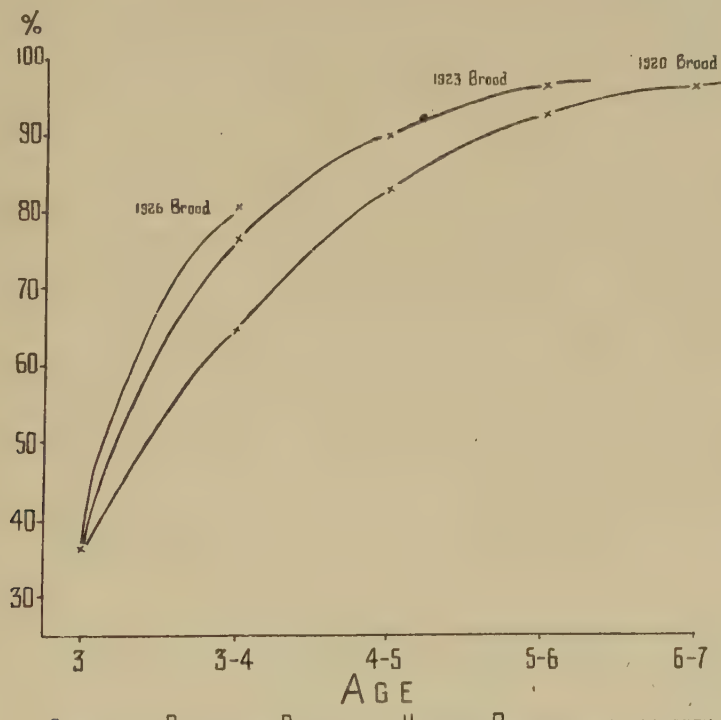


Fig. 5.

mature. Haddock We are also probably justified in concluding that up till the time (occurring, on the average, early during the third year of life) that a size of from 26 to 28 cms. is reached, and escape through the trawl meshes ceases to be possible, the mortality rate is much lower (for the purpose of these calculations it will be assumed to be 20 per cent., although this assumption will require to be justified or modified from the evidence of better data). The change over to a rate about three times as large is probably almost entirely due to the attainment of a size where escape through the net ceases to be possible and the fish become the direct object of intense commercial fishing. This change, while it gradually takes effect during the third year, is first made fully apparent in the results expressing the relative sizes of the catches of the fourth year age-classes.

Assuming these two rates of elimination to be those actually taking effect we

find that for every 100 Haddock available for capture in the first year of life there would be available, from the second to the eighth year respectively, 80, 68, 26, 12, 6, 3 and 1, so that under present conditions the numbers of a brood have, by about the seventh year of life, become so attenuated as to be of little significance. Indeed, after the fourth year of life, even a prolific brood has lost the dominating influence it has, exercised for about a couple of years.

For the designation of the ratio of the numbers of fish surviving from one year to another Lea (4) uses the symbol ϱ, and for that of the numbers removed from the stock the symbol δ, where $\varrho = 1 - \delta$. In order to avoid repetition of terms these symbols will be used here, δ and ϱ being, however, expressed as percentage numbers instead of as fractions of unity (i.e. $\varrho = 100 - \delta$).

We may now enquire how, if at all, the general trend of the value of δ indicates that the equilibrium between the available Haddock stock and the crop taken from it is in course of change. The value for ϱ for each of the years of life after the attainment of maturity can be calculated from Table C, I for each of the three most productive recent broods for which we have data, viz:— those of 1920, 1923 and 1926. We will assume that $\varrho = 64$ at the end of the third year of each brood — i.e., that δ has been at the rate of 20 per cent. in passing from the first to the second and from the second to the third years of life. (As a matter of fact the value of δ showed some irregularity in these cases, but this may be ignored since it is the effect of the inroads made by the fishery on the mature fish of marketable size which we wish to trace). Table C, II shows the value for ϱ for the three broods. The cumulative wastage per original 100 fish (i.e. the differences between 100 and the successive values of ϱ) is plotted out in Fig. 5, from which it appears that there is a distinct suggestion that the 1920 brood was less intensively operated upon than that of 1923, and the latter in turn — as far as the data exist for it — than that of 1926. If this is so then within the last decade the tendency has increased to crop out the chief Haddock broods at an accelerated rate. One corollary of such a tendency should be seen in a reduction of the average size of the fish landed, and this reduction has indeed taken place, since, to take only the case of the trade category "Extra-Small Haddock", it is now found that more of such fish go to the cwt. than was hitherto the case.

2. The apparent Rates of Depletion (of recent Haddock Broods) in the three main Growth-Rate Areas of the North Sea.

The early distribution of the various new broods is not always over a similar area of the North Sea, with the result that there is some variation in the mean growth rates and the precise ages at which these broods first become the object of the commercial fishery. A brood which is well distributed towards the southern portion of the North Sea will grow, on the whole, more rapidly than one mainly confined to the northern and deeper waters, and will in consequence suffer from earlier exploitation. It should be of interest and importance then to determine whether there is in the data an indication of differential mortality rates for the chief Haddock sub-areas of the North Sea. One would

TABLE D

RECENT NORTH SEA HADDOCK BROODS: PERCENTAGE LOSS OR GAIN IN NUMBERS WITH INCREASING AGE

[Treating the three Main Sub-Areas separately]

I CENTRAL AREA (Area c)

BROOD	APPARENT LOSS OR GAIN BETWEEN AGES:-								
	A	B	C	D	E	F	G	H	J
	1 to 2	2 to 3	3 to 4	4 to 5	5 to 6	6 to 7	7 to 8	8 to 9	9 to 10
1919	+ 86	- 30	- 63	- 72	- 61	- 82	- 70	- 57	- 44
1920	+ 5	- 24	- 46	- 72	- 77	- 65	- 68	- 65	- 100
1921	+ 54	- 2	- 67	- 20	- 51	- 84	- 15	- 100	
1922	+ 122	- 53	- 54	+ 18	- 85	- 10	- 100		
1923	+ 41	- 38	- 66	- 78	- 65	- 81			
1924	+ 28	- 11	- 68	- 66	- 75				
1925	+ 104	- 39	- 65	- 70					
1926	- 38	- 16	- 68						
1927	- 55	- 8							
1928	- 6								
	+ 34%	- 25%	- 62%	- 51%	- 69%	- 64%	- 63%	- 74%	- 72%

AVERAGE

← – – – – – – – – AVR. 65% – – – – – – – – →

II S. EASTERN AREA (Area E)

1919	- 42	- 27	- 50	- 52	- 10	- 30	- 80	- 83	- 100
1920	- 55	0	- 48	- 40	- 71	- 12	- 93	- 12	- 89
1921	+ 59	+ 133	- 54	- 81	+ 87	- 75	- 100		
1922	+ 100	+ 131	- 72	- 29	- 83	- 10	- 56		
1923	- 55	- 22	- 64	- 64	- 87	- 50			
1924	+ 55	+ 6	- 82	- 68	- 29				
1925	+ 217	- 35	- 82	- 52					
1926	- 51	+ 4	- 81						
1927	+ 17	- 2							
1928	+ 61								
	+ 31%	+ 21%	- 67%	- 55%	- 32%	- 35%	- 82%	- 47%	- 95%

AVERAGE

← – – – – – – – – AVR 59% – – – – – – – – →

III N. WESTERN AREA (Area W)

1919	- 56	- 29	- 62	- 49	- 68	+ 31	- 91	+ 18	- 92
1920	+ 29	- 37	- 44	- 35	- 45	- 77	+ 37	- 97	- 52
1921	+ 7	+ 97	- 8	- 62	+ 2	+ 57	- 95	- 80	
1922	+ 650	- 16	- 14	- 11	+ 88	- 83	- 94		
1923	+ 150	+ 42	- 63	- 39	- 87	- 71			
1924	+ 229	- 1	- 26	- 78	- 60				
1925	0	+ 2	- 46	- 50					
1926	- 35	- 27	- 54						
1927	+ 507	- 34							
1928	0								
	+ 150%	± 0%	- 40%	- 46%	- 28%	- 29%	- 61%	- 53%	- 72%

AVERAGE

← – – – – – – – – AVR 47% – – – – – – – – →

expect that the apparent rate for *Area c* — the deep central portion of the sea, which is mainly a nursery for small Haddock which subsequently desert it for the other areas where better conditions for growth exist — should rule higher than the general average. It would also be anticipated that the rate for the north-western area — *Area W* — should

be lower than the average, since this area gains at the expense of Haddock forsaking *Area c* and even to some extent (after each northerly spawning migration) the south-eastern *Area E*.

The data for these three areas separately are given in Table D. The average δ for mature fish is, as might be anticipated, much higher than the general North Sea average in the case of *Area c* ($\delta = 65$) and slightly higher in that of *Area E* ($\delta = 59$); for *Area W* it is much smaller ($\delta = 47$). The results for the immature fish are also significant. For all three areas there appear to be more fish available for capture in the second year than in the first (δ in this case having a positive value, which is of course absurd). Especially is this the case in *Area W*, which, although it is a spawning area, has only been extensively colonised by brood Haddock once (in 1928) in recent years. It is in the second year of life that a new brood generally migrates into this area (at the expense of *Area c*). Probably the chief cause of the prevailing positive value of δ in column A lies in the fact that, in its first year of life (and even partly in its second) the Haddock does not fully abandon the pelagic for the demersal mode of life, and so is not taken in representative numbers in the trawl. This would be particularly true, one would think, in the nursery ground which *Area c* constitutes, and would suffice to explain the positive value for that area, which is the only area showing loss of numbers in column B. The positive values for *Area E* in columns A and B — at least that in the latter column — are probably expressive of a definite influx of Haddock from *Area c* to the shallower waters of the south-east. The influx into *Area W* from *Area c* in the second year (column A) is probably continued on a smaller scale in the third year, when (column B) the data suggest that something like a balance exists between fish immigrating and fish removed.

In making forecasts of the probable North Sea yield it will obviously be essential, then, to take into account the early distribution of each new brood, since the mortality rate and the ages at which a brood will give its greatest yield will depend to some extent upon whether that brood had initially a northerly or a southerly cast as compared with the normal distribution of new Haddock broods. It should be possible, as data accumulate from year to year, to place these determinations on a fairly accurate basis; and, if the sources of the data could be more fully tapped, the mortality rates for still smaller subdivisions of the North Sea might be calculated. Indeed, for the purpose of making forecasts of local yield it will probably be found necessary more fully to utilise such sources of data as are available.

3. The probable Significance of the higher Age-Composition of the Haddock Stock on Rockall Bank.

Although commercial vessels have now begun to visit Rockall Bank that area has not till very recently been subjected to the inroads of such a continuous fishery as occurs in the North Sea. It would be ideal if in our studies of rates of depletion of Haddock stocks we could make some preliminary observations upon a virgin stock. As no such stock is now available the next best thing is to consider the age-composition of stocks such as that of Rockall, which is, from the commercial viewpoint, probably being only partially exploited. In May, 1929, scale samples were taken from 220 Rockall

Haddock which were typical of three trawl catches landed from that area in Aberdeen. At the same time the necessary data were obtained for calculating the size distribution of the catch per 100 hours' fishing, whence, from the scale-age determinations, the catch of each age-group in unit time was determined. These age-group frequencies are set forth in Table C, IV, and for comparison, the frequencies at corresponding stages of life of the 1920 brood — a typical North Sea Haddock Brood — are given. The Rockall stock of 1929 obviously contains a much higher proportion of older Haddock than is now found in the North Sea; indeed, if the total weight of fish caught in unit time were compared with that from the latter area the difference would be very striking. It is of course probably true that, as has been stated before (2) the initial numerical strength (per unit of area) of a new brood at Rockall is greater than that of a North Sea brood. The major portion of the difference in the relative numbers of older fish is, however, likely due to the fact that the mortality rate is (under present conditions) lower at Rockall than in the North Sea. Thus if we smooth the Rockall age-group frequencies in threes and calculate the percentage diminution of numbers from year to year, we obtain, for a period extending from the fourth to the fourteenth year of life, the serial values 38, 48, 46, 31, 12, 13, 39, 14, 83 for δ — or an average δ value of 32. The similar value for the 1920 North Sea brood (from the third to the tenth year) is 64. These figures are probably sufficiently typical to afford a rough indication of what proportion of the stock is being fished out in each of the two self-contained areas considered. The presence of such a large proportion of older fish at Rockall introduces (or brings into greater prominence) the question of the relative importance of competition for food as a factor affecting stock equilibrium. As far as can be judged from those catches at Rockall in 1929 the annual increase in size averages only three centimetres after the age of five years is attained, and the maximum size reached (just over 70 cms.) is fully 30 cms. less than that attained by the oldest Iceland Haddock (also of about 14 years of age). This maximum size of Rockall Haddock has persisted since 1908, when the "Dana" conducted fishing experiments on the Bank. So plentiful are the older fish that the commercial vessels do not find it necessary to land Haddock which have not reached their fourth year of life, whereas, in the case of the North Sea, Haddock of the second year are landed, often in great numbers. It will be interesting to observe whether an increased intensity of fishing, should the latter materialise, will in the course of time bring back the average age of the youngest Haddock landed from Rockall to a lower limit.

One can merely speculate as to the part played by the relatively considerable number of older Rockall Haddock in maintaining the existing high level of regenerative capability.

4. Actual Yield and Yield Forecasted on the Basis of the apparent Mortality-Rate.

From the foregoing it is evident that no single formula can be derived to serve as the basis of estimates of the future yields of Haddock broods in different regions — or even in natural sub-divisions of the same region. Allowance may require to be made in the factor expressing the general mortality-rate, in the case of any sub-area, according

to the relative intensity of the fishing operations to which that area is exposed, and according to the balance existing between emigration and immigration. While admitting without reserve that a vast amount of work remains to be done on accumulated and improved data for all Haddock regions before accurate forecasts of yield can be made for these regions, we may proceed, in the case of North Sea Haddock, for which the best data exist at present, to improvise and test an experimental formula for the purpose.

In the case of recent North Sea broods (Table B) it was found that, by virtue of even the limited degree of sampling which has been available, the relative frequencies of the age-groups have been determined with sufficient accuracy to yield, when multiplied by the appropriate mean weights of the individual fish, figures closely approximating to those of the actual catch by weight (in the same unit time). This result was reached if the assumption were made that only half the available Haddock of the second year of life were retained by the trawl (or actually landed). We will continue to make this assumption for the present, although an enquiry will require to be made as to the exact proportion of the available second year fish (and indeed, of third year fish in areas of slow growth) which is marketed and so included in the commercial statistics of weight. The total theoretical yield of the fishery in unit time in any single year of operation was therefore obtained by summing the weight-products arrived at for the various age-classes present. The successive year-classes may be designated as a, b, c.....n where a represents the oldest age-class present. To represent the frequencies of these year-classes (i.e. the average numbers in which they are caught in unit time) in the various years of life we may make use of the index figures 1 to 7, so that a_7, a_6, a_5, etc. represent the frequency of brood a in the seventh, sixth, fifth etc. years of life. As before, brood frequencies in years subsequent to the seventh will, on account of their relative insignificance, be ignored. The typical commercial catch (ignoring fish rejected) in unit time will therefore be represented in the year for which the study commences by:—

(1) $$a_7 + b_6 + c_5 + d_4 + e_3 + {}^1/_2 f_2$$

and that for the following year by:—

(2) $$b_7 + c_6 + d_5 + e_4 + f_3 + {}^1/_2 g_2$$

In (2) since the apparent value for ϱ (the percentage number of fish surviving from the previous year, and $= 100 - \delta$) is 44 for all year-classes except the second, where it is probably about 80,

$$b_7 = 0.44\ b_6;\ c_6 = 0.44\ c_5 \ldots\ldots\ldots f_3 = 0.44\ f_2;\ \text{and}$$
$$g_2 = 0.80\ g_1.$$

Since only the value ${}^1/_2 g_2$ (i.e. $0.40\ g_1$) is to be taken, (2) becomes (if the substitution of $0.44\ g_1$ for $0.40\ g_1$ be allowed)

(3) $$0.44\ (b_6 + c_5 + d_4 + e_3 + f_2 + g_1).$$

To obtain the weight-products for each of these frequency designates we may introduce the symbols $W_1, W_2, W_3 \ldots\ldots\ldots W_7$ for the mean weights of North Sea Haddock in their first, second, third.....seventh years respectively, so that (3) becomes

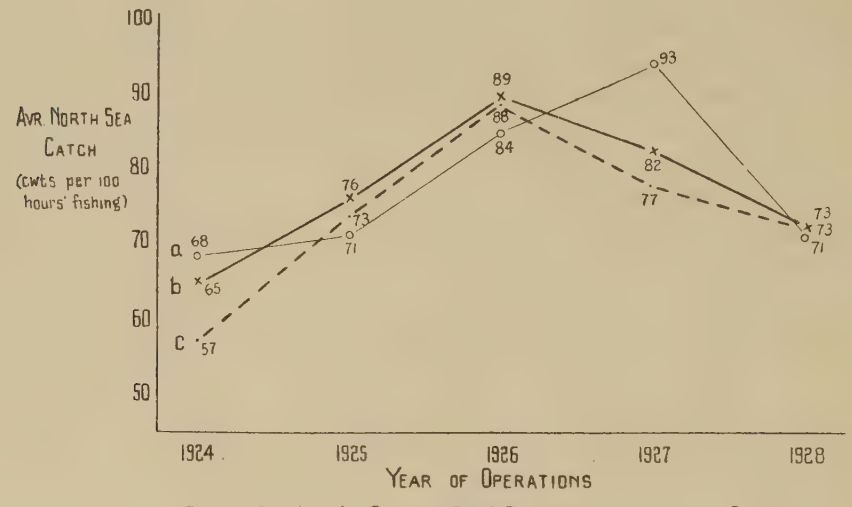
$$(4) \qquad 0.44\ (W_7\ b_6 + W_6\ c_5 + W_5\ d_4 + W_4\ e_3 + W_3\ f_2 + W_2\ g_1) =$$

the weight which should be caught by commercial vessels in unit time. This formula — whatever be its accuracy — is the simplest which can possibly be devised for forecasting the coming year's yield from the known empirical brood frequencies in the current year, since it is based on the assumption that, on the average, the same value of δ is applicable at all stages after maturity is reached. To attempt to calculate the anticipated yield during any one calendar month of the coming year, for example, we should have to introduce a modifying factor, since, as the graphical representation of the weights caught monthly shows (Fig. 3) similar weights are by no means likely to be caught in different months of the year; further, the W factors would require modification to agree with the respective weights reached by the various age-classes in the particular month of the year being considered. For seasonal forecasts of the probable catches in sub-areas account would require to be taken of the growth rate as well as of the normal seasonal variations in the density of the Haddock shoals in these areas. In short, the compilation of a considerable number of reference tables — of the "ready reckoner" type — would require to be undertaken before the task of forecasting for local fishing grounds was entered upon.

In Table B a comparison is made between the theoretical yields (calculated from the known frequencies of the recent broods) and the corresponding yields actually obtained in each of the recent years. We may now test the efficacy of the formula given above, which is presumedly to enable us to calculate the yield for the coming year from the hypothetical frequencies of the broods in that year. These frequencies will be, in this case, those found for the year just closed but modified by an average factor which makes allowance for reduction of brood numbers in the interim. This average factor was obtained, as has been shown, by averaging the apparent rates of survival of many more broods than are now to be considered so that in a sense we are also testing the validity of applying this average factor to particular cases.

In making these calculations we must, according to the formula, make use of the frequencies of the appropriate first year age-classes. As has been mentioned, it is not always easy to obtain accurate estimates of these particular frequencies. We may therefore make the calculations using in each case two values for the first-year age-class concerned — a) the actual first year brood-frequency determined from research vessel catches, and b) the actual frequency of the brood determined from the same source in the second year — that for which the calculation is being made. This latter frequency value, though known in these test instances, would not of course be known in practice. It is introduced here to show if and to what extent our present estimates of the survival of new broods will throw us out in our calculations. The results then obtained compare as follows with the actual fishery results:—

Year of operation	1924	1925	1926	1927	1928
a) Estimated catch (cwts. per 100 hours).....	68	71	84	93	71
b) Estimated catch (cwts. per 100 hours).....	65	76	89	82	73
c) Actual catch (cwts. per 100 hours)	57	73	88	77	73



a. Forecasted Yield, using First Year Brood Frequencies in last term of Formula
b. " " using Second Year " " " " " " "
c Actual Fishing Yield

[Forecast Calculations a. and b made from formula of the type

$$0{\cdot}44\,(W_7b_6 + W_6c_5 + W_5d_4 + W_4e_3 + W_3f_2 + W_2g_1)$$

where $W_2 - W_7$ = mean weights of Haddock in Years 2-7

and $g_1 - b_6$ = frequencies of age-classes 1-6 in year previous to that of operations]

Fig. 6.

These results are displayed in graphical form in Fig. 6. There is remarkably close agreement between b) and c) whereas a) (obtained by including the empirical first year Haddock frequencies) gives misleading results. It is probable then that much more care will require to be bestowed on the estimation of the frequency of broods during their first year of life, since misleading estimates at this stage can throw forecasts well wide of the mark. Given improved data for this stage it would appear to be possible, even at present, to make accurate forecasts of the mean catches (by weight) which will be made in the North Sea generally in the coming year.

(In comparing the close agreement just obtained between theoretical and actual yields for these years with the less complete agreement obtained in the case of the calculations made earlier in this paper (Table B, II), we may readily discern the cause of the discrepancy. In the case of the latter estimate it was stated that by smoothing the

calculated annual weights in threes a much closer agreement could have been made to exist. Now in carrying out the calculations which yielded the results just considered we have in effect introduced a certain degree of smoothing, since the weight-estimates were made from the modified brood-frequencies of the previous year, whereas in the other case they were made from the frequencies of the current year).

D. On the Determination of the Causes of Fluctuations.

Apart from promising to serve utilitarian purposes the studies of fluctuations in the stocks of food-fishes justify themselves on purely scientific grounds since, in seeking after the causes of these fluctuations we are bound to follow a rational scheme of investigation focussed on the reactions of the fish-life to the animate and inanimate environment. It is possible that the first hint of the probable course of fluctuations may yet come from timely hydrographic observations or from plankton studies carried out before, during, and after the fish's breeding season (although the precise forecasts of yield must inevitably be made from brood-frequency estimates). While it is essential, for practical reasons alone, to bear in mind the necessity of ascertaining the causes of fluctuations, it is quite probable that the actual technique of forecasting could be worked out while these causes remain undiscovered. It seems probable that if the facts relating to the incidence and the precise extent of fluctuations were well-established for the commercially more important fish species, research into the causes of fluctuations could be the better and more economically pursued. If a tabulation be made of the successful and unsuccessful broods of the different species (and races) of food-fishes at present under consideration the result is a bewildering ensemble in which it is impossible to detect the influence of any common causative factor. This is so even if we so tabulate for only one fish — the Haddock — in the different regions over which it is distributed. Yet if we are to come by the general causes of fluctuations it seems more likely that we shall do so first by way of determining what causes, in one and the same year, a high rate of brood survival of a fish species in one region, and a moderate or low rate in another. No broad study of causative factors need be inaugurated which cannot be sustained over a period of years. It is difficult to see how a satisfactory programme based on quantitative plankton studies, with hydrographical correlations, could be undertaken. What could, however, first of all be attempted, were the means available, is a full study of all the possible factors in a circumscribed area in which fluctuations are marked. In the case of the Haddock such an area is to be found to the north-west of Scotland. In this area there is each year a spawning concentration of Haddock, but only in one of the recent years (1928) was the area at all densely restocked with brood fish after the spawning season. It is probable, on all the evidence, that the products of this spawning area are normally drifted north-eastwards towards and even beyond the Shetlands. At all events here is an isolated but important spawning ground near the mainland where an intensive study is practicable of the conditions prevailing in the overlying waters and of the numbers and fate of the eggs, larvae and fry resulting from each of a series of spawning seasons.

If it be decided, however, that only general hydrographical and plankton studies should or can be continued, then it appears to be essential that the annual variations in the movements of Atlantic high-salinity water should be charted as well as those of the chief current systems of the North Sea (Poulsen (*5*) has traced a connection between the drift of Haddock larvae and an unusually early inflow of Atlantic water into the Belt Sea). At the same time it should be part of a broad programme to endeavour to determine each year the time of incidence and the extent of the outcrop of the unicellular organisms which are probably the chief food of the post-larval fish.

E. Summary and Conclusions.

It is certain that the study of fluctuations of fish stocks is one which if it is to be effective invokes a good deal of international collaboration. As far as can at present be seen in the case of Haddock, there is a case for the extension of the means of obtaining the fundamental data. Two valuable sources of data have been dealt with above. The first, resulting from the activities of the research vessels, is not fully exploited in the international sense. The second, the statistics of the results of the commercial fishing, is, from the point of view of the contribution it could make towards sustaining a forecasting service, capable of improvement in the following particulars — 1) Where measurements of Haddock are made, it is desirable to have collected the material for the age-determination of these Haddock; 2) The weight of Haddock caught in unit time, and the gross weight landed, refer to the rectangles comprising only the North Sea proper, and might with advantage be returned also for the area to the west of Scotland. No such regular returns are at present available for other areas carrying an independent Haddock stock — viz. Rockall, Faroe, Iceland and the Norwegian Plateau, nor, in the case of the North Sea, are the Haddock statistics of countries other than Great Britain readily available. There is not carried on, in the sense that it is carried on for the North Sea Haddock, a regular biological survey of the Haddock stocks of these other regions; 3) The promptitude with which the data are made available. Some priority of treatment would require to be accorded to the statistics of those species whose probable yields are to be the subjects of prediction; 4) Where statistics are to be communicated only in the form of compilations it is a desideratum that such compilations should be made for the natural year (i.e. April to March in the case of Haddock) and not for the calendar year.

How, if the making of predictions is to become speedily practicable, these extensions in and alterations of the methods of securing the essential data are to be carried out, is a matter for international discussion. By the term "prediction" is here meant not the broadcasting of anticipations of increased or reduced catches, but of actual calculations of the probable yield such as will be publicly expected. In the case of Haddock it has been shown above that such calculations could probably be made for the general area of the North Sea even if but a slight improvement in the data could possibly be effected. For the preparation of detailed local forecasts for different sub-areas the data would require to be very much improved upon (as an alternative to allowing for a further period of accumulation and study of data collected as at present).

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III. PLAICE

Die Erforschung der Fluktuationserscheinungen bei der Scholle (*Pleuronectes platessa* L.).

Von

Adolf Bückmann, Helgoland.

Das Vorkommen von Fluktuationen bei der Scholle ist seit langem nachgewiesen. In den Jahren 1893—99 beobachtete Apstein, dass die kleinen Schollen des ersten Lebensjahres an der Küste der westlichen Ostsee in sehr verschiedener Menge auftraten. Er brachte diese Erscheinung in Beziehung zu dem hydrographischen Zustand des Gebiets in den einzelnen Jahren, dem er einen Einfluss auf die Bruterzeugung zuschrieb, und glaubte die Wirkung der »Fluktuationen« in den Erträgen der Fischerei einige Jahre später wiedererkennen zu können. In dieser Arbeit aus dem Jahre 1905 hat also Apstein den ganzen Umfang des Fluktuationsproblems bereits erkannt.

Seither sind Fluktuationen bei der Scholle in allen Gebieten und von den verschiedensten Forschern nachgewiesen worden. Mit verbesserten Methoden hat A. C. Johansen die Dichte der Schollen des ersten Lebensjahres an den dänischen Küsten verfolgt (Johansen 1909, 1927, Bruun 1927). Strodtmann und Langhammer untersuchten die Alterszusammensetzung des Schollenbestandes in der westlichen und mittleren Ostsee in den Jahren 1921—23, als sich in diesem Gebiet noch viele alte Schollen fanden. Sie zeigten, dass in ihrem Material die Abkömmlinge gewisser Jahrgänge viel häufiger waren als die von anderen, jüngeren Jahrgängen, und erkannten so einige Jahrgänge als gut, andere als schlecht. Einige Jahre darauf fanden etwa gleichzeitig Blegvad in der Ostsee und der Übergangsarea, Bückmann, Thursby-Pelham in der Nordsee, A. R. Molander im Bornholmbecken, A. Vedel Tåning bei Island durch Untersuchung der relativen Alterszusammensetzung des Schollenbestandes in aufeinanderfolgenden Jahren, dass Fluktuationen in den genannten Gebieten vorkamen und auf den Bestand einen sehr merklichen Einfluss hatten. Aage J. C. Jensen beurteilte den Ausfall der einzelnen Jahrgänge auf Grund der Fangdichte pro Stunde im Hornsriffgebiet. Thursby-Pelham untersuchte in der Nordsee, Johansen und Blegvad in der Übergangsarea, Molander und Kändler in der Ostsee den Einfluss der Fluktuationen auf den Ertrag der Fischerei.

Bedeutung der Untersuchungen.

Aus mehreren Gründen ist eine genaue Untersuchung der Fluktuationen sehr erwünscht.

1) Die Auswirkungen der Fluktuationen überlagern die fortgesetzten, gleichgerichteten Veränderungen des Bestandes, die durch die Fischerei und gegebenenfalls durch die Veränderungen im Fischereibetrieb (Ehrenbaum und Kyle 1930) entstehen. Will man solche Veränderungen bleibender Art erkennen, so muss man in der Lage sein, die kurzfristigen, zufälligen Veränderungen des Bestandes, die durch die Fluktuationen hervorgebracht werden, rechnerisch auszuschalten.
2) Es ist von praktischer Bedeutung, den Einfluss der Fluktuationen auf den Ertrag der Fischerei zu kennen und diesen Ertrag vorhersagen zu können.
3) Es ist von grösster wissenschaftlicher Bedeutung und kann sich als praktisch wichtig erweisen, die Ursachen der Fluktuationen aufzudecken.

I. Die Methoden zur Erkennung des Ausfalls der Jahrgänge.

Studium der relativen Alterszusammensetzung.

Die bisherigen Untersuchungen, vor allem in der Nordsee, beweisen, dass man durch die Kontrolle der relativen Alterszusammensetzung des Schollenbestandes die grossen Züge der Fluktuationen richtig erkennen kann. Die endgültige Bestätigung hierfür brachten die Marktuntersuchungen von Thursby-Pelham, aus deren Berechnungen hervorgeht, dass die als arm erkannten Jahrgänge wirklich der Fischerei einen viel geringeren Ertrag lieferten als die reichen. Trotzdem hat die Methode mancherlei Mängel. So ist sie z. B. nicht geeignet, um das zahlenmässige Verhältnis der Dichte zweier Jahrgänge zu ermitteln. Nicht einmal bei solchen Jahresklassen, die gleichzeitig gefangen werden können, ist das möglich. Die Prozentsätze der Jahresklassen geben ja nur die Anteile am Bestande an; also Teile einer Grösse, die von Jahr zu Jahr wechselt. Bei Helgoland ist im Sommer und Herbst gewöhnlich die III-Gruppe am häufigsten, sie bildet etwa $^1/_3$ des Bestandes, während auf die IV-Gruppe nur etwa ein Viertel kommen. Befindet sich nun eine besonders arme Jahresklasse in der III-Gruppe, wie 1926, so ist der Bestand geringer als normal, und wenn nun die IV-Gruppe sogar die Hälfte des Bestandes stellt, so folgt daraus keineswegs, dass sie das doppelte der durchschnittlichen Dichte erreichte. — Besondere Schwierigkeiten bietet die Methode in einem Gebiet, wo sich der Bestand nur aus wenigen jüngsten Altersgruppen zusammensetzt, wie z. B. gegenwärtig im Herbst vor der Ostfriesischen Küste. In den deutschen Untersuchungsserien können wir verfolgen, wie der Bestand hier seit 1925 von Jahr zu Jahr jünger geworden ist (Bückmann 1930) und im Herbst 1929 wurden sogar mit dem verhältnismässig weitmaschigen Netz des »Poseidon« fast nur noch Schollen der I- und II-Gruppe gefangen (77 % bzw. 19 %). Wenn der Zustand, dass die I-Gruppe alle älteren beiweitem überwiegt, in diesem Gebiet bestehen bleibt, so ist es sehr schwer, wonicht unmöglich, hier die Stärke der Jahrgänge zu beurteilen, weil zu wenig Vergleichsmöglichkeit gegeben ist. Ähnliche Schwierigkeiten bieten sich, nach Blegvads Untersuchungen zu urteilen, etwa im nördlichen Kattegat. — Sollen schliesslich zwei Jahrgänge verglichen werden, die nicht gleichzeitig gefangen worden sind, so lässt uns die Methode vollkommen in Stich. Es sind Anzeichen vorhanden, dass der Bestand in der Deutschen Bucht, soweit es sich um Schollen von der III-Gruppe an aufwärts handelt,

sich von 1924 bis 1929 auf etwa die Hälfte oder noch weniger verringert hat. Ein Versuch, die Dichte des Jahrgangs 1920 und 1925 miteinander zu vergleichen unter Zugrundelegung der relativen Häufigkeit der IV-Gruppe 1924 bzw. 1929, muss zu einem ganz falschen Ergebnis führen.

Im günstigsten Fall führt uns also das Studium der relativen Alterszusammensetzung zur ganz allgemeinen Charakterisierung der Jahrgangsdichte durch Ausdrücke wie »reich«, »arm«, »normal«. Es ist aber für den Fortschritt unserer Kenntnis der Fluktuationen von entscheidender Bedeutung, zu einem genaueren, zahlenmässigen Vergleich der einzelnen Jahresklassen zu kommen. Es leuchtet ein, dass eine unscharfe Bestimmung der Häufigkeit eines Jahrgangs eine sehr ernste Fehlerquelle bei der Vorhersage des Fangertrages bedeutet. Ein guter Jahrgang zeichnet sich durch seine relative Häufigkeit in einer Reihe von schlechten viel stärker ab als zwischen normalen und guten Jahrgängen. Die Gefahr liegt nahe, im ersteren Falle einen viel günstigeren Fangertrag vorauszusagen als berechtigt ist. — Auch für die Analyse der Ursachen der Fluktuationen ist die genaue Kennzeichnung der Stärke der einzelnen Jahrgänge nötig. Nur dann ist es möglich, in der von A. C. Johansen angegebenen Weise durch die Korrelationsrechnung zu prüfen, ob dieser oder jener Faktor einen Einfluss auf den Ausfall der Jahrgänge hat. Hierüber wird weiter unter mehr zu sagen sein.

Fig. 1. Alterszusammensetzung von Schollenfängen in der Deutschen Bucht (»Poseidon«, Mai 1929). Die Abbildung zeigt die grosse Ungleichmässigkeit der Häufigkeit der einzelnen Altersgruppen selbst auf nahe benachbarten Stationen.
weiss = Altersgruppe I; längs weit schraffiert = Altersgruppe II; schräg schraffiert = Altersgruppe III; längs eng schraffiert = Altersgruppe IV; schwarz = Altersgruppe V+.
Die Position der kleinen Fänge unter 100 Stück durch Punkte bezeichnet.

In einigen anderen Fällen, so vor allem beim Norwegischen Hering und beim Donegalhering haben allerdings langjährige Untersuchungen der relativen Alterszusammensetzung wertvolle Ergebnisse über die Stärke der Jahrgänge, die Zehrungskurve etc. geliefert. In diesen Fällen war es aber verhältnismässig leicht möglich, Altersproben zu sammeln, die für den ganzen Bestand bzw. die biologische Gruppe der Laichfähigen repraesentativ waren. Das ist im Falle der Scholle schwierig oder unmöglich. Die Alterszusammensetzung ist fast an jeder Stelle des Verbreitungsgebiets eine andere, alle biologischen Gruppen werden vermischt gefangen (Fig. 1). Will man die für einzelne Gegenden gewonnenen Teilwerte zu einem

Gesamtbild vereinigen, so muss man die lokale Bestandsdichte in Betracht ziehen, und damit geht man von der Methode, nur die relativen Häufigkeitswerte zu verwenden, bereits ab.

Die Untersuchung der Dichte und ihre Schwierigkeit.

Es muss also in Zukunft der Versuch gemacht werden, nicht den Anteil der Jahresklassen an dem (veränderlichen) Bestande zu bestimmen, sondern ein Mass für die absolute Häufigkeit zu gewinnen. Als solches haben wir den Durchschnittsfang je Zeiteinheit des Grundschleppnetzes anzusehen, das ja zum Fang von Schollen aller Grössen hervorragend geeignet ist. Nun ist aber die Dichte eines Jahrgangs bekanntlich keineswegs über sein ganzes Verbreitungsgebiet hin gleich. Junge Schollen sind nahe der Küste, ja ganz dicht am Strand besonders reichlich, ältere Schollen im Gegensatz dazu im Küstengebiet spärlich, in den tieferen zentralen Gebieten im Verhältnis häufiger, und man ist nicht in der Lage, eine Zone anzugeben, in der eine gewisse Altersgruppe in repraesentativer Dichte gefangen werden könnte. Zudem machen die Schollen jahreszeitliche Wanderungen senkrecht zur Küste, und wir können annehmen, dass diese, durch klimatische Faktoren etc. beeinflusst, nicht in allen Jahren gleich verlaufen. Auch mag daran erinnert werden, dass Abwanderung der älteren Schollen in die Tiefe vor den verschiedenen Abschnitten der Küste in verschiedenem Tempo zu erfolgen scheint, so z. B. gegenwärtig in dem Abschnitt Schiermonnikoog—Norderney schneller als in dem Abschnitt Hever—Lister Tief. Endlich finden wir in den letzten Untersuchungen einige Anzeichen dafür, dass ein Jahrgang in einem Teilgebiet, ein anderer im anderen besonders häufig oder spärlich sein kann, und es ist möglich, dass derartige Unterschiede sich von den jüngsten Bodenstadien bis etwa zum Eintritt der Reife erhalten. Einige Beobachtungen in dem neueren deutschen Material lassen sich durch die Annahme solcher Verschiedenheiten innerhalb der Nordsee am leichtesten erklären. Stellt man ferner zusammen, was sich aus den Angaben der verschiedenen Autoren über die Stärke der Jahrgänge entnehmen lässt, so erkennt man gleichfalls Unterschiede der Dichte gewisser Jahrgänge in den Teilgebieten der Nordsee. Wir geben nur einige Beispiele:

Gebiet	Haaks-Leman	Helgoland	Hornsriff
Jahrgang 1921	sehr gut, besser als 1920 und 1922,	gut, aber ärmer als 1922	ärmer als 1920 und 1922
— 1923	ärmster beobachteter Jahrgang,	ärmster beobachteter Jahrgang,	normal, etwa wie 1921,
— 1924	arm, merklich besser als 1923,	arm, merklich besser als 1923,	ärmster beobachteter Jahrgang, viel ärmer als 1923.

Sollen also Dichtebestimmungen vorgenommen werden, so müssen die Proben so verteilt werden, dass aus allen diesen Verschiedenheiten regionaler Art keine Fehler entstehen.

Dichtebestimmungen an den Schollen der 0-Gruppe.

Die Fehlerquellen der wechselnden Tiefenverbreitung werden auf ein Minimum reduziert, wenn man mit A. C. Johansen die Dichtebestimmungen an den Jahresklassen im Sommer des ersten Lebensjahres vornimmt. Zu dieser Zeit sind die Schollen noch auf einem schmalen Streifen entlang der Küste von der Strandlinie bis etwa zur 3 m-, allenfalls der 5 m-Linie zusammengedrängt, d.h. also auf einem Streifen von ungünstigstenfalls einigen 100 m Breite. Hier können repraesentative Proben natürlich leichter gewonnen werden als auf den weiten Flächen der offenen See.

Die Schollen der 0-Gruppe werden nach Johansen in diesem Küstenstreifen von kleinen flach gehenden Motorbooten aus mit einem kleinen Grundschleppnetz bestimmter Art und Abmessung gefangen. Der durchschnittliche Stundenfang in dem untersuchten Gebiet ist ein Mass für die Dichte des Jahrgangs. Johansen hat sich selbst eingehend mit den Fehlerquellen dieser Methode auseinandergesetzt. Wir kennzeichnen die kritischen Bedenken hier ganz kurz:

1) Innerhalb ihres Verbreitungsgebiets sind die Schollen der 0-Gruppe sehr ungleichmässig verteilt. Es ist daher schwieriger als man annehmen sollte, repraesentative Zahlen zu erhalten.
2) Die Fangkraft der einzelnen Netzindividuen ist trotz der angestrebten Gleichheit verschieden.
3) Es ist sehr schwierig, die Fahrtgeschwindigkeit beim Schleppen gleich zu halten, zumal bei den wechselnden Stromverhältnissen an der Küste. Durch die Fahrtgeschwindigkeit wird aber die Fangkraft des Netzes beeinflusst und die Grösse der abgefischten Fläche ist von ihr abhängig.
4) Das Wetter und die Durchsichtigkeit des Wassers können einen sehr stark wechselnden Einfluss auf den Fang haben.

Trotzdem kann Johansen eine Reihe von Beweisen für die Brauchbarkeit der Resultate seiner Untersuchungen anführen. In mehreren Fällen, wo das Auftreten besonders reicher Brutjahre in der 0-Gruppe festgestellt worden war, hatte die Fischerei in der Folge eine Periode reicher Fänge, und zwar gerade nach Ablauf der Zeit, die den Schollen unter den jeweiligen Verhältnissen nötig war, um zu marktfähiger Grösse heranzuwachsen. Daher dürfen wir es als erwiesen ansehen, dass man mit Hilfe der Johansenschen Methode die Grundzüge der Fluktuationen richtig erkennt. Indessen wäre es doch erwünscht festzustellen, in wiefern die durch sie gewonnenen Zahlenangaben zuverlässig sind. Ein Weg zur Kontrolle ist es zu vergleichen, ob die Dichte der 0-Gruppe eines Jahrgangs mit der I-Gruppe hinreichend übereinstimmt. Prüft man das Verhalten der 0- und I-Gruppe an den von Johansen 1927 für die Beltsee mitgeteilten Zahlen, so findet man eine sehr befriedigende Übereinstimmung. Der Korrelationskoeffizient, der sich für die 8 möglichen Kombinationen ergibt, beträgt

$$\varrho = + 0{,}820 \pm 0{,}116.$$

Bei Vereinigung der Befunde aus der Beltsee und dem südlichen Kattegat in der von Johansen angegebenen Weise ergeben sich 11 Vergleichsmöglichkeiten von 0- und

I-Gruppe. Die Korrelation ist dabei minder scharf, doch ist ihre Existenz noch wahrscheinlich:

$$\varrho = + 0{,}448 \pm 0{,}241.$$

(Die Vereinigung beider Reihen ist also nicht empfehlenswert.) Im nördlichen Kattegat und im Skagerrack finden wir bei entsprechenden Untersuchungen keinen Anhalt für die Existenz einer Korrelation. Wahrscheinlich liegt die völlig ausreichende Erklärung dafür in der von JOHANSEN festgestellten Tatsache, dass junge Schollen des ersten Lebensjahres in wechselnder Menge aus dem Skagerrack in das nördliche Kattegat einwandern. — In der Beltsee stimmen JOHANSEN's Zahlen aus den letzten Jahren auch erträglich mit denen überein, die BLEGVAD für den Durchschnittsfang der I-Gruppe in tieferem Wasser gefunden hat. Wir geben nach JOHANSEN die relativen Häufigkeitswerte, die er durch Berücksichtigung von 0- und I-Gruppe erhält. Auf diese Weise konnte er Bestimmungen für eine fortlaufende Reihe von Jahrgängen geben, obwohl 1923 keine Untersuchungen gemacht waren. BLEGVAD's Zahlen wurden dadurch erweitert, dass die Dichte des Jahrgangs 1921 durch Umrechnung aus der Häufigkeit der II-Gruppe abgeleitet wurde ($^2/_3$ dieser Zahl):

Jahrgang	1921	1922	1923	1924	1925
JOHANSEN	27,6	23,0	46,8	32,7	49,8
BLEGVAD	9,2	2,7	23,6	3,3	24,3

Es zeigen sich also dieselben Jahresklassen als arm bzw. reich. Der zahlenmässige Unterschied zwischen arm und reich ist aber bei BLEGVAD viel grösser.

Im Zusammenhang mit der Analyse der Ursachen der Fluktuationen zeigt JOHANSEN, dass eine sehr scharfe Korrelation zwischen der Stärke der Jahrgänge und den hydrographischen Bedingungen in dem Gebiet während des pelagischen Lebens der jungen Schollen besteht. Dies positive Ergebnis spricht zweifellos für die Brauchbarkeit der Johansenschen Methode, wenn man auch natürlich keinen Kreisschluss machen darf. Nun ist es ganz interessant zu bemerken, dass JOHANSEN's Zahlen der Jahrgangsstärke 1921—25 eine sehr gute Übereinstimmung mit der Salzgehaltsanomalie zeigen (der Korrelationskoeffizient beträgt $+ 0{,}66 \pm 0{,}23$) während andererseits BLEGVAD's Zahlen sich besser dem Verlauf der Temperaturanomalien anpassen ($\varrho = + 0{,}863 \pm 0{,}106$).

Wir kommen also vorläufig zu dem Ergebnis, dass die Methodik der Dichtebestimmungen an der 0-Gruppe noch der Vervollkommnung bedarf, dass vor allen Dingen die Untersuchungen stets in möglichst grossem Umfang ausgeführt werden müssen, um zufällige Fehler, die sich bei der ungleichmässigen Verteilung der kleinen Schollen leicht einstellen, nach Möglichkeit auszugleichen. Wenn es nicht möglich ist, die einzelnen Küstenabschnitte gleichmässig mit Proben zu belegen, so ist zu empfehlen, die örtlich gewonnenen Zahlen zu wägen entsprechend der Küstenlänge, die sie repraesentieren sollen. Wenn man bedenkt, dass jede Methode der Dichtebestimmung grosse Fehlerquellen in sich schliesst, dass wir ferner eine zweite erprobte Methode der Fluktuationsuntersuchung der Johansenschen nicht an die Seite zu stellen vermögen, so kann man ihre Anwendung nur empfehlen.

Auch in der Nordsee könnten mit Hilfe derselben wesentliche Probleme gelöst werden. So wäre es z. B. wünschenswert zu wissen, welche Bedeutung die einzelnen Küstenabschnitte für die Erneuerung des Bestandes im tieferen Wasser haben, ob z. B.

Fig. 2. Ausdehnung und Vielgestaltigkeit des von Schollen des ersten Lebensjahres besiedelten Gebiets in der Deutschen Bucht.
Das Gebiet zwischen der 0 m- und der 5 m-Linie ist punktiert, ---- 10 m-Linie, ·—· 20 m-Linie, —· 40 m-Linie.

an der holländischen Westküste die Schollen der 0-Gruppe in derselben Häufigkeit vorkommen wie an den Küsten der Deutschen Bucht, ob sich an den glatten Seeküsten

der Inseln ebensoviel von ihnen finden wie im Wattenmeer oder in den Mündungen der grossen Ströme. Auch könnte man feststellen, ob die Unterschiede der Häufigkeit in verschiedenen Teilgebieten, die sich bei manchen Jahresklassen finden, auf ihrer Verteilung schon im ersten Lebensjahr bei der Einwanderung in das Küstengebiet beruht und dergl. mehr.

Dagegen sind die Bedingungen in der Nordsee, speziell an der deutschen Küste, wenig günstig, um regelmässig Jahr für Jahr die Stärke der Jahrgänge auf dem 0-Gruppenstadium zu ermitteln. Die technischen Schwierigkeiten sind vor allem im Wattenmeer ausserordentlich gross. Der schmale Streifen vom Strand bis zur 5 m-Linie, innerhalb dessen sich die jungen Schollen im wesentlichen aufhalten, ist hier zerteilt und aufgelöst durch die vielen Tiefs und Seegats und die zahllosen weitverzweigten kleinen Priele (Fig. 2). Die Strandlinie bei Niedrigwasser ist um ein Vielfaches grösser als die eigentliche Küstenlinie und unvergleichlich viel grösser als die Kontur der Küste, wie sie etwa von dem Inselsaum gezeichnet wird. Die Untersuchungen können nur bei Niedrigwasser und etwa 1—2 Stunden vor- und nachher ausgeführt werden, denn während des hohen Wasserstandes verteilen sich die jungen Schollen auf dem überfluteten Watt. Statt eines schmalen Streifens sind nun weite Flächen von ihnen bevölkert, Flächen, auf denen noch dazu im allgemeinen nicht mit dem Boot gearbeitet werden kann. Auch machen die Tidenströme das Fischen mit dem Jungfischnetz schon kurze Zeit nach Stauwasser unmöglich, da dann die Grösse der in der Zeiteinheit abgefischten Fläche nicht mehr reguliert werden kann. Dabei sind die Bodenverhältnisse und daher auch die Besiedlung mit Schollen in unserm Watt noch wechselnder als an der glatten jütischen Küste, die Zahl der Stichproben muss hier also entschieden noch grösser sein als dort. An den einzelnen Stationen muss zunächst durch Fischen in mehreren Tiefenstufen die dichtestbesiedelte Zone aufgesucht werden. Weiter sind die Fänge im oberen, mittleren und unteren Teil der grösseren Priele recht verschieden gross. Wegen der obenerwähnten Tidenverhältnisse kann man in vielen Fällen an einem Arbeitstag nur ein Priel erledigen. In einem Watten-Teilgebiet müssen aber mehrere solcher Priele untersucht werden, ausserdem wenigstens an einigen Stellen die Ränder des grossen Tiefs oder Seegats. Die Mitteilungen über die deutschen Erfahrungen mit dem Jungfischtrawl, soweit sie nicht eigenen Beobachtungen entstammen, verdanke ich Dr. Kieckebusch, der mir freundlichst die Einsicht in ein noch unveröffentlichtes Manuskript gestattete. Bei im Laufe des Jahres wiederholt ausgeführten Untersuchungen zeigte es sich übrigens, dass die grösste Fangmenge an 0-Gruppe in einem Falle im August, in anderen im Herbst oder gar im folgenden Frühjahr erzielt wurde, sodass es vielleicht nicht möglich ist, durch einmalige Untersuchung zu brauchbaren Werten zu gelangen. Es sind daher Untersuchungen von solchem Umfange nötig, dass die Durchführung auf sehr grosse Schwierigkeiten stossen würde, zumal ja die ganze Küste untersucht werden müsste. Daher kann für die Nordsee die Johansensche Methode als Standard für die laufende Kontrolle der Fluktuationen nicht empfohlen werden.

Dichtebestimmungen an älteren Schollen.

Als zweiter Weg bleibt die Möglichkeit, die Dichte der Jahrgänge in höherem Alter, also ausserhalb der Strand- und Wattenzone, auf den Jungfischgründen festzustellen. Die Arbeiten müssen sich aber hier über ein grösseres Areal erstrecken.

Es bestehen viele Bedenken dagegen, die beschränkte Zahl von Stichproben, die die Forschungsfahrzeuge uns liefern können, als Grundlage für Dichtebestimmungen zu nehmen. Als zuverlässiger werden im allgemeinen Dichtebestimmungen auf Grund des Durchschnittsfanges der Fischerfahrzeuge angesehen, da sich diese auf ein viel grösseres Material gründen. Man könnte nun einmal an den am Markt gelandeten Fischen umfangreiche Altersbestimmungen anstellen und die erhaltenen Zahlen entsprechend dem Durchschnittsfang je 100 Stunden in den Teilgebieten, aus denen die Proben stammen, wägen. Daraus würde sich ohne weiteres die Dichte der einzelnen Jahresklassen in den Teilgebieten berechnen lassen. Da aber bei der Altersbestimmung der Wert der Schollen leider erheblich vermindert wird und umfangreiche Altersbestimmungen an Marktmaterial daher sehr teuer werden, so könnte man den zweiten Ausweg benutzen, die Dichte der Jahrgänge durch Kombination der relativen Alterszusammensetzung nach Forschungsdampferfängen und des Durchschnittsfanges der Fischer im gleichen Gebiet zu errechnen. Ein Vergleich der Messungsreihen der wissenschaftlichen Fänge und der Marktfänge würde zeigen, welcher Teil der Urfänge wirklich gelandet worden ist.

Eine nähere Diskussion dieser beiden Möglichkeiten können wir uns ersparen, denn weder das ganze Gebiet noch ein Teilgebiet wird so gleichmässig befischt, dass der Durchschnittsfang ein brauchbares repraesentatives Dichtemass wäre. Die Fischer konzentrieren ihre Anstrengungen auf die Stellen, wo die besten Nutzfischfänge gemacht werden. Im Mai 1929 fischte praktisch die ganze Deutsche Kutterflotte bei Amrum. Die »Poseidon«-Untersuchungen der Häufigkeit der Altersgruppen zu dieser Zeit auf verschiedenen Schnitten der Deutschen Bucht zeigten (Fig. 1), dass man zu bedeutenden Fehlschlüssen kommen würde, wenn man den hier erzielten Durchschnittsfang von Schollen etwa der Altersgruppen IV—VII als repraesentativ für die Deutsche Bucht, ja auch nur ihren nördlichen Teil ansehen würde. Zudem könnte eine gute Bestimmung der Dichte der einzelnen Jahrgänge lediglich auf Grund von Marktuntersuchungen z. B. in der Nordsee erst in ziemlich hohem Alter, also viel zu spät, erfolgen.

So bleibt wohl für die Nordsee kein anderer Weg, als Dichtebestimmungen auf Grund von Forschungsdampferfängen zu machen. Es muss eben eine Methode gefunden werden, die bekannten Fehlerquellen so klein wie möglich zu machen.

Allen Bedenken kann man entgegenhalten, dass Blegvad in der Beltsee und dem südlichen Kattegat bereits mit Erfolg solche Bestimmungen vorgenommen hat, dass Jensen auf gleiche Weise zu brauchbaren Vorstellungen von der Stärke der Jahrgänge gelangte und dass von den schottischen Forschern auf ganz dieselbe Weise Dichtebestimmungen beim Schellfisch ausgeführt wurden, deren Ergebnisse sich als durchaus zuverlässig erwiesen haben. Dabei ist das Areal, das beim Schellfisch in Betracht gezogen werden muss, weit grösser als das wichtige Schollenfanggebiet.

Die räumliche Ausdehnung der Untersuchungen kann bei der Scholle sehr eingeschränkt werden. Es handelt sich ja darum, die Dichte möglichst auf jungen Alters-

stadien zu bestimmen. Daher kann man sich auf die eigentlichen Jungfischgründe beschränken, die z.B. in der Deutschen Bucht zwischen der Küste und der 30 m-Linie liegen. In diesem Gebiet fängt man den wesentlichen Teil der Schollen der ersten drei Lebensjahre, ja im Frühjahr sogar der ersten 5 Lebensjahre, derart, dass die Menge der weiter seewärts lebenden Individuen dieser Gruppen trotz der Grösse des Areals annähernd vernachlässigt werden kann.

Durchschnittlicher Stundenfang von Schollen innerhalb und ausserhalb des Gebiets der dichten Frühjahrsansammlungen an der Küste.

	innerhalb	ausserhalb
Mai 1924	2000	5,5
Mai 1929	1634	12,9
Juni 1930	1496	23,5

Es ist also anzunehmen, dass man ein recht gutes Bild von der Dichte des Bestandes erhält, wenn es nur gelingt, das Gebiet dieser Anhäufung von Schollen ausreichend mit Proben zu belegen. In den Gebieten westlich von Borkum Riff und der flämischen Bucht ist die Zusammendrängung der Schollen an der Küste im Mai minder scharf ausgeprägt. Ob sie hier bereits früher im Jahr eintritt, oder gar nicht, kann ich nicht beurteilen. In dem (wahrscheinlicheren) letzen Falle müsste man sich damit abfinden, hier ein breiteres Gebiet zu untersuchen.

Die Brauchbarkeit der Dichtebestimmungen hängt nun m. E. wesentlich von der Verteilung der Untersuchungsstationen ab. Noch so umfangreiche Fangversuche an einzelnen begrenzten Stellen führen nach unseren Erfahrungen zu falschen Schlüssen und lassen sich nicht zu einem Gesamtbild vereinigen. Gute Erfolge dagegen verspreche ich mir, wenn man das ganze Untersuchungsgebiet mit einem Netz von Einzelfängen überzieht, die in möglichst gleichmässiger Weise auf Schnitten senkrecht zur Küste angeordnet sind. Ein Beispiel für eine solche Untersuchung bietet die Fahrt des »Poseidon« in der Deutschen Bucht im Mai 1924 (Fig. 3). Damals wurden leider noch nicht auf allen Stationen Altersbestimmungen ausgeführt. Dass dies aber an sich möglich ist, beweisen unsere Erfahrungen auf der Maifahrt von 1929. Es wurden zwar in der Deutschen Bucht weniger, im ganzen aber etwa ebensoviel Fänge gemacht wie 1924. Kleinere Fänge wurden ganz auf Alter untersucht, von grösseren eine Probe von 500—1000 Stück. Eine Probe von 1000 Stück dürfte auch bei sehr grossen Fängen genügen. Die Bestimmungen wurden gleich an Bord ausgeführt.

Verschiedene Methoden sind denkbar, wie aus den auf den einzelnen Stationen gefundenen Dichtewerten der Altersgruppen die Häufigkeit derselben im ganzen Gebiet abgeleitet werden kann. Wenn das Netz der Untersuchungen gleichmässig genug ist, könnte man die Summe aller Fänge als repraesentativ ansehen, anderenfalls die auf jedem Schnitt, ja auf jeder Station gefangenen Zahlen wägen entsprechend der Grösse des Areals, für das sie als Probe gelten. Dagegen würde ich es nicht für zweckmässig halten, in Anlehnung an die von Buchanan-Wollaston entwickelte Methoden Umrisslinien gleicher Häufigkeit für jede Altersgruppe zu ziehen und die darin befindliche

Gesamtzahl unter Benutzung des Areals innerhalb der Umrisslinien zu berechnen. An sich wäre auch das technisch durchführbar, doch werden unsere Zahlen niemals so genau sein, um die Arbeit lohnend erscheinen zu lassen.

Leider ist ein Beweis dafür, dass eine solche Untersuchung zum Ziele führen würde,

Fig. 3. Lage der Kurrzüge der Fahrt des »Poseidon« in der Deutschen Bucht im Mai 1924. Fänge von über 100 Stück je Stunde mit einem, Fänge von über 1000 Stück mit zwei Querstrichen. 10 m-Linie, ·—· 20 m-Linie, ---- 30 m-Linie, ··—·· 40 m-Linie.

die Dichte der einzelnen Jahrgänge einigermassen richtig zu schätzen, noch nicht zu erbringen. Nur ein Versuch kann darüber entscheiden. Ein Umstand scheint dafür zu sprechen. Nach einem rohen Verfahren, unter Anwendung geradliniger Interpolation, habe ich versucht, aus den Fängen des »Poseidon« von Juli 1923 und Mai 1924 den

Gesamtbestand der Schollen in der Deutschen Bucht innerhalb gewisser Grenzen zu schätzen; wie man es theoretisch erwarten sollte, waren die resultierenden Zahlen einander sehr ähnlich. Wenn aber die Schätzung des Gesamtbestandes nach einem derartigen Verfahren brauchbar ist, so darf man hoffen, dass dies auch für die Schätzung der einzelnen Gruppen gilt.

Vor den Dichtebestimmungen an der 0-Gruppe hat das hier vorgeschlagene Verfahren den Vorzug, dass man die gefundenen Zahlen durch den Vergleich benachbarter Jahresklassen in mehreren aufeinanderfolgenden Jahren kontrollieren kann. Fehlbestimmungen, die in einem Jahr vielleicht infolge ungünstiger Wetterbedingungen erfolgt wären, würden in den folgenden Jahren noch erkannt werden können.

Der Zehrungskoeffizient.

Beschränkt man, wie es hier vorgeschlagen wird, die Untersuchungen auf die Jungfischgebiete, so hat man den Nachteil, dass man aus den gewonnenen Zahlen nicht zugleich Aufschluss über die Grösse der Zehrung erhält. Die Verminderung eines Jahrgangs innerhalb eines Jahres stellt nämlich auf den Jungfischgründen eine komplexe Erscheinung dar, die sich aus der Zehrung und der Abwanderung nach tieferen Gebieten zusammensetzt. Eine rohe Vorstellung von der Abnahme der Zahl mit zunehmendem Alter erhält man für ein begrenztes Gebiet, indem man das Mittel aus den hier beobachteten relativen Frequenzen der verschiedenen Altersgruppen aus einer grossen Zahl von Jahren zieht. Auf diese Weise wird der Einfluss der Fluktuationen zum grössten Teil aufgehoben, und man erhält ungefähr eine »normale« Abnahme von einem Jahr zum anderen. Für die Scholle haben wir eigentlich noch nicht ausreichend lange Beobachtungsreihen. Macht man aber einen Versuch mit dem vorhandenen Material (Helgoland und der Schnitt Haaks-Leman), so erhält man ganz gut übereinstimmende Werte. Vergleicht man aber diese mit den Werten, die Thursby-Pelham mit Hilfe ihrer Marktanalysen gefunden hat[1]), so zeigt sich, dass im Marktmaterial die älteren Gruppen viel häufiger sind und entsprechend der Verminderungskoeffizient zwischen zwei Altersgruppen geringer ist. Das kann garnicht überraschen, denn die Fischerei findet auch auf den zentralen Gründen statt, wo die älteren Schollen von der Küste aus zuwandern und wo infolgedessen der Verminderungskoeffizient kleiner ist als die Zehrung. Wenn auch die Küstengebiete genügend befischt werden, so mag sich die Überschätzung der Zehrung auf diesen und die Unterschätzung derselben auf zentralen Gründen gegeneinander ausgleichen. Für die jüngsten von der Fischerei mit gelandeten Altersgruppen dürfen wir allerdings nicht erwarten, richtige Zehrungskoeffizienten aus dem Marktmaterial zu erhalten. Ein grosser Teil der Schollen der Altersgruppen II—IV hat noch nicht marktfähige Grösse, von den jüngeren wird jeweils ein grösserer Teil weggeworfen, und so erhalten wir aus den Marktanlandungen eine Zunahme der Zahl von der III- auf die IV-Gruppe statt einer Abnahme, und es ist wahrscheinlich, dass auch der Verminderungskoeffizient zwischen IV- und V-Gruppe noch kleiner ist als die wirkliche Zehrung.

[1]) Durch Berechnung der Abnahme des Durchschnittsfanges eines Jahrgangs im Laufe eines Jahres.

Zehrung in Prozent.

Altersgruppe...	III	IV	V	VI	VII
Helgoland, IX	27	48	67	77	
Helgoland, VII	27	50	62	76	
Haaks-Leman	26	54	61	74	
Lowestoft, Markt	..	37	53	52	

Die Bestimmung der Zehrung aus wissenschaftlichen Fängen ist nur möglich, wenn auch die zentralen Gründe in derselben Weise repraesentativ befischt würden wie die Küstengebiete. Das würde eine gewaltige Mehrarbeit bedeuten. Zweckmässiger ist es wohl, die Zehrung zwischen den älteren Gruppen in der von Thursby-Pelham angegebenen Weise aus den Marktuntersuchungen zu ermitteln, über deren grosse Wichtigkeit sogleich noch gesprochen werden muss.

II. Internationale Zusammenarbeit zur Voraussage der Fischereierträge etc.

Wir wenden uns nun der Frage zu, wie das Fluktuationsproblem bei der Scholle durch internationale Zusammenarbeit seiner Lösung näher gebracht werden und die erzielten Ergebnisse praktisch nutzbar gemacht werden können.

Die notwendigen Arbeiten zerfallen in zwei natürliche Gruppen, die Kontrolle der Stärke der einzelnen Jahrgänge und Untersuchungen am Markt zur Feststellung des Einflusses der Fluktuationen auf den Fang.

Zusammenarbeit zur Untersuchung der Jahrgangsstärke.

Es wurde oben vorgeschlagen, für den ersten Zweck in der Nordsee Untersuchungen auf den Jungfischgründen über die Häufigkeit der Schollen der Altersgruppen II—IV anzustellen. Das Gebiet der dichten Ansammlungen dieser Jungschollen sollte alljährlich im Frühjahr mit einem möglichst dichten und gleichmässigen Netz von Stationen überzogen werden, die auf Schnitten senkrecht zur Küste angeordnet sind. An jeder dieser Stationen sollte ein einstündiger Grundschleppnetzfang gemacht und die erbeuteten Schollen auf Alter untersucht werden. In Fig. 4 ist das Gebiet, das zur Untersuchung empfohlen wird, abgegrenzt und durch Schraffierung bezeichnet. Die ungefähre Zahl und Lage der Schnitte ist durch starke Linien senkrecht zur Küste bezeichnet. Dies Gebiet könnte unter die Nationen, die an der Untersuchung teilzunehmen beabsichtigen, aufgeteilt werden. Sehr günstig für die Untersuchungen würde es sein, wenn man sich von vornherein über einige technische Einzelheiten einigen könnte. Da kleine Fische, von der II-Gruppe an, gefangen werden sollen, so müssen engmaschige Netze verwendet werden, am besten wohl in Gestalt eines Doppelsacknetzes in der von Davis angegebenen Form. Weiter sollte eine Standarddauer der Züge (1 Stunde) und eine Normalgeschwindigkeit beim Kurren ($2^1/_2$ Sm.-Stunde) ausgewählt werden. Die Ergebnisse würden doppelt wertvoll sein, wenn sie mit einem Netz gleicher Grösse erzielt würden, da dann die Zahlenwerte ohne Weiteres vergleichbar wären. Es ist zu empfehlen, ein kleines Netz

von etwa 50 Fuss Kopftaulänge zu wählen, um die Mitarbeit auch derjenigen Staaten zu sichern, deren Forschungsfahrzeuge nicht für das grosse Fischdampferschleppnetz eingerichtet sind.

Benutzt man die Häufigkeit älterer Schollen zur Bestimmung der Jahrgangsdichte, so ist darauf zu achten, ob nicht Veränderungen im Fischereibetrieb (Zu- oder Abnahme der Befischung, Verwendung weitmaschigerer Netze, Übergang zur Wadenfischerei) auftreten, die die beobachteten Häufigkeiten beeinflussen können.

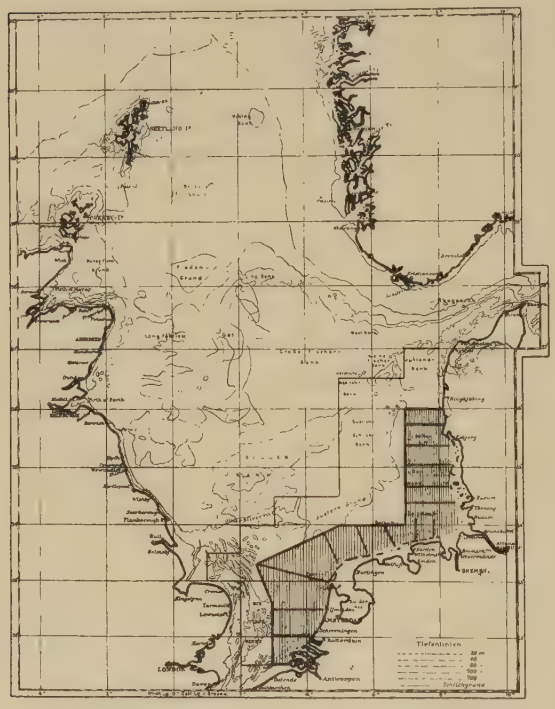

Fig. 4. Ausdehnung des zur Untersuchung empfohlenen Gebiets (schraffiert) und Lage der wichtigsten Schnitte (starke Linien) in demselben. Durch eine dünne schwarze Linie begrenzt das Gebiet bedeutender Schollenfänge (über 20 Cwts in 100 Trawlstunden im Jahre 1925 nach Thursby-Pelham 1928).

Was nun andere wichtige Schollengebiete anlangt, so sind die Küsten des Kattegats, der Beltsee und der Ostsee für die Anwendung der Johansenschen Methode des Fanges der 0-Gruppe geeigneter als die der Nordsee. Im Falle des nördlichen Kattegats ist aber zu bedenken, dass durch Johansen's und Bruun's Untersuchungen die Einwanderung einjähriger Schollen vom Skagerrack her nachgewiesen ist. Diese Einwanderung ist von wechselnder Stärke, und wenn man die Feststellung der Jahrgangsstärke als Grundlage für die Voraussage des Ertrages benutzen will, so muss sie frühestens bei der I-Gruppe erfolgen. Diese wird auch ausserhalb der Küstenzone in grosser Menge gefangen, ja sie ist in Blegvad's Material (1927) stets die häufigste. Auf Grund dieser Angaben würde man es für empfehlenswert halten, regelmässige Untersuchungen mit dem Aal-Tog über die Dichte der I-Gruppe anzustellen.

Bruun schliesst aus dem Missverhältnis von 0- und I-Gruppe, dass die Einwanderung kleiner Schollen sich in manchen Jahren bis ins südliche Kattegat und in die Beltsee erstrecken kann. Das ist jedoch zweifelhaft. Eine Einwanderung von Skagerrackschollen in diese Gebiete von solchem Ausmass, dass erheblich mehr I-Gruppe gefangen würde als im vorhergehenden Jahre 0-Gruppe, müsste sich an den Rassencharakteren der Schollen, der Strahlenzahl der Analflosse, erkennen lassen. Johansen findet in der Strahlenzahl zwar Schwankungen, aber doch geringere als in solchem Falle zu erwarten wären. Allerdings fehlen Untersuchungen in den entscheidenden Jahren. Nach den Nordsee-Erfahrungen aber bin ich geneigt anzunehmen, dass das von Bruun festgestellte Missverhältnis in der 0- und I-Gruppen Häufigkeit durch geringen Umfang der Unter-

suchungen sehr viel zwangloser zu erklären ist als durch Einwanderung. Wir haben im Elbgebiet einen ganz parallelen Fall festgestellt. Im Gebiet der Beltsee scheint daher der Ausfall der Jahrgänge durch 0-Gruppenuntersuchungen am besten zu ermitteln. Die Untersuchung an älteren Stadien verbietet sich schon wegen der in diesem Gebiet ausgeführten Aussetzungen von Nordsee- und Limfjordschollen. Durch diese Aussetzungen ist unsere Frage aber für dies Gebiet, in dem internationale Zusammenarbeit ohnehin nicht infrage kommt, ziemlich gegenstandslos geworden.

In der eigentlichen Ostsee ist die Zahl der 0-Gruppen-Schollen, die am Strande gefangen wird, verhältnismässig gering. Nach Mitteilung von Dr. Fischer ist zwar der Ausfall der Jahrgänge aus ihrer Häufigkeit klar zu erkennen, doch würde die Genauigkeit der Feststellung zunehmen, wenn man auch hier die Dichte der nächstälteren Jahrgänge (I und II) auf den Jungfischgründen untersuchte. Als solcher kommt an der Deutschen Küste vor allem die Oderbank infrage.

Die Marktuntersuchungen.

Um der Voraussage des Fangertrages näher zu kommen, ist es unerlässlich, die Alterszusammensetzung der angelandeten Fänge genau zu untersuchen, und zwar bei jedem Betriebe, für den man eine Voraussage wünscht. Wenn nämlich der Einfluss der Fluktuationen auf den Fangertrag vorausgesagt werden soll, so muss man zunächst genau wissen, welche Altersgruppen in der Anlandung vorkommen, wie gross im Durchschnitt der Anteil der einzelnen Altersgruppe am Gesamtfang nach Zahl und Gewicht ist, und sodann muss der Einfluss der Fluktuationen eine Zeitlang empirisch festgestellt werden.

Wir müssen uns jedoch einmal die Frage vorlegen, bis zu welchem Grade denn bei der Scholle der Fangertrag durch die Fluktuationen bestimmt wird. Es ergibt sich ohne weiteres aus theoretischer Überlegung, dass der Einfluss bei einem stark befischten und raschwüchsigen Bestande, wo die Fischerei sich nur auf einige wenige Jahrgänge stützt, am stärksten sein muss. Solche Verhältnisse liegen z. B. in der Übergangsarea vor, wo die dänische Schollenfischerei die bedeutendste ist (Tabelle). In den Erträgen der dänischen Kattegatfischerei verursachte der gute Ausfall des Jahrgangs 1925 eine merkliche Steigerung (1927), als die Schollen dieses Jahrgangs im Alter von 2 Jahren marktfähig wurden. In der dänischen Fischerei in der Beltsee und seit 1924 auch in der Ostsee überwiegt die fallende Tendenz alle anderen Bewegungen, und Schwankungen unter dem Einfluss der Fluktuationen sind nicht zu bemerken. In der Beltsee dürfte dies wenigstens z. T. darauf zurückzuführen sein, dass in den letzten Jahren gute und schlechte Jahrgänge in regelmässigem Wechsel standen, sodass der Fischerei entweder eine reiche II-Gruppe oder eine reiche III-Gruppe zur Verfügung stand. (Blegvad 1927.) In der Ostsee haben wir es dagegen mit der Abfischung eines angehäuften Bestandes zu tun. Am Ende dieser Entwicklung steht ein Zustand starker Beeinflussung des Ertrags durch die Fluktuationen, wie die neuen Untersuchungen von Kändler (1930) gezeigt haben. Die deutschen Fischer sahen sich gezwungen, nach Abfischung des angehäuften Bestandes zur Ausnutzung immer jüngerer Altersklassen überzugehen, obwohl das Wachstum hier noch nicht so schnell ist wie in der Übergangsarea. Im Jahre 1927 war

Ertrag der dänischen Schollenfischerei in t.

Jahr	Kattegat	Beltsee	Ostsee
1921	1999	1745	569
1922	2015	1577	650
1923	2022	1363	1267
1924	1858	1295	2009
1925	2187	1226	1639
1926	2242	1084	1293
1927	**3191**	833	812
1928	2194	689	568

Ertrag der deutschen Schollenfischerei in der Ostsee (in t) nach Kändler.

		1923	1924	1925	1926	1927	1928	1929
Ganzes Ostsee-Gebiet		2594	1909	2784	2841	2319	4301	2028
Vorpommern		1946	1070	1711	1713	1458	3566	?
Vorherrschende Jahrgänge in Altersgruppe	II	..	..	..	..	..	..	1927
	III	..	..	1922	1923	..	1925	..
	IV	..	..	**1921**	1922	..	..	..

der schlechte Jahrgang 1924 in der III-Gruppe der Gegenstand der Fischerei, und die Fänge waren daher gering. Im folgenden Jahre (1928) stützte sich die Fischerei auf den guten Jahrgang 1925 und erzielte reiche Erträge. 1929 befand sich wieder ein schlechter Jahrgang in der III-Gruppe, nämlich 1926. Dementsprechend waren die Fänge gering. Allerdings wurden im Herbst bereits die grössten Individuen der Altersgruppe II (Jahrgang 1927) für den Markt brauchbar, da das Minimalmass von 18 cm noch in Geltung war. Doch konnten diese kleinen Fische noch keinen sehr grossen Ertrag bringen. Die Folge dieser Situation war nun aber, dass der Jahrgang 1927 scharf befischt wurde und im Jahr 1930 in stark gelichteter Zahl in die III-Gruppe eintritt, sodass Kändler nicht glaubt, einen guten Fangertrag für dieses Jahr voraussagen zu können.

Was die Fischerei in der Nordsee anbetrifft, so machen Ehrenbaum und Kyle (1930) darauf aufmerksam, dass der Gesamtschollenfang in der Nordsee von jeher sehr geringe Schwankungen aufgewiesen hat. Das ist leicht verständlich, da sehr viele Altersgruppen zum Fange beitragen. Thursby-Pelham unterscheidet 9 Gruppen und eine Sammelgruppe der Älteren, in der augenscheinlich noch eine grössere Zahl von Gruppen vertreten ist. Diese Gruppen sind bemerkenswert gleichmässig am Fange beteiligt, was schon daraus hervorgeht, dass 1928 die Gruppen von VI an mit $^1/_3$ an der Zahl und mit nicht viel weniger als $^2/_3$ am Gewicht der gefangenen Schollen beteiligt sind. Unter diesen Umständen kann der Ausfall eines einzelnen Jahrganges den Fang nur in recht geringem Masse beeinflussen. Zudem hat es den Anschein, als ob die Amplitude der Schwankungen, der Unterschied zwischen dem besten und dem schlechtesten Jahrgang in unserm Gebiet nicht allzu stark, z. B. merklich geringer als beim Schellfisch wäre. Auch scheint es so, als ob der Zeitabstand zwischen erfolgreichen Brutjahren gering wäre (1922—1925—1928). Merklicher muss der Einfluss der Fluktuationen auf eine Fischerei sein, die sich auf weniger Altersgruppen

stützt, wie die Deutsche Kutterfischerei in der Deutschen Bucht und anscheinend zum grössten Teil auch die dänische Snurrewadenfischerei. Immerhin kommen auch hier mehrere Altersgruppen (Deutsche Kutter Mai 1929 IV—VIII)[1]) in Betracht und manchmal stellen verhältnismässig alte Schollen den grössten Teil des Fanges (Mai 1929 7-jährige Schollen ca. 35 %)[1]).

Alterszusammensetzung der Schollenanlandungen in Lowestoft. Fang von Dampftrawlern je 100 Fischstunden (nach Thursby-Pelham).

Altersgruppe		I	II	III	IV	V	VI	VII	VIII	IX	X	XI+
Dez. 1927.	Stück	—	184	453	416	1318	721	245	134	41	34	132
	Cwts	—	0,4	1,3	1,7	7,0	5,5	2,7	1,8	0,7	0,7	3,2
	Jahrgang	*1926*	*1925*	*1924*	*1923*	*1922*	*1921*	*1920*	*1919*	*1918*	*1917*	*1916—*
Dez. 1928.	Stück	30	516	1107	577	264	616	343	127	68	36	74
	Cwts	0,1	1,3	3,6	2,3	2,0	5,5	3,7	1,8	1,3	0,7	1,7
	Jahrgang	*1927*	*1926*	*1925*	*1924*	*1923*	*1922*	*1921*	*1920*	*1919*	*1918*	*1917—*

Wir wissen ferner, dass auch andere Faktoren ausser den Fluktuationen den Fangertrag beeinflussen. Zu nennen ist hier in erster Linie die Wachstumsgeschwindigkeit. Schon Johansen (1908) hat nachgewiesen, dass die fluktuierenden Schwankungen des Wachstums von Bedeutung für den Fang der dänischen Kutter sind. In Jahren guten Wachstums wachsen mehr Schollen aus der grossen Masse der Untermassigen zu

Schollenfang je Reisetag. Holländische Dampftrawler in der Nordsee.

	kg
1921	274
1922	241
1923	209
1924	217
1925	233
1926	267
1927	*247*

Schollenfang der deutschen und dänischen Kutter in der Nordsee in t.

Jahr	Dänemark	Deutschland
1922	5808	824
1923	6011	1093
1924	6794	1170
1925	14013	1436
1926	14857	2004
1927	**11370**	**1803**
1928	17646	2005
1929	..	2107

[1]) Geschätzt auf Grund der vom Poseidon auf den Hauptfangplätzen festgestellten Alterszusammensetzung von Schollen über 25,0 cm.

marktfähiger Grösse heran und können gefangen werden als in schlechten Wachstumsjahren. Bei der dänischen und der deutschen Kutterfischerei in der Nordsee beobachten wir 1927 eine Verminderung der Erträge gegenüber 1926 und 1928, und dasselbe zeigt sich in dem Fang je Reisetag der holländischen Dampftrawler. Die Ursache hierfür können wir nicht in den Fluktuationen suchen. 1927 ist das Jahr, in dem wir den grössten Ertrag von dem guten Jahrgang 1922 hätten erwarten sollen, der jetzt in der V-Gruppe stand. Im folgenden Jahr aber, das den höheren Fangertrag brachte, wurden sowohl die V- als auch die IV-Gruppe, die einen wesentlichen Anteil am Fang der deutschen Kutter zu haben pflegen, von schlechten Jahrgängen (1923 und 1924) gebildet. Aber 1927 war ein schlechtes Wachstumsjahr, 1928 ein gutes, wie man durch Messung der Ringbreiten in den Schollenstatolithen und durch die direkte Berechnung der mittleren Längen der einzelnen Altersgruppen beweisen kann. Es ist also wahrscheinlich, dass in diesem Jahr die Wachstumsgeschwindigkeit für den Ausfall der Fanges bestimmend gewesen ist.

Breite der Ringe im Schollenstatolithen. Mittelwerte aus je 50 Messungen.

Statolithenzuwachs des 3. Lebensjahres:

im Jahre	1924	14,8	Mikrometerteile
- —	1925	8,7	—
- —	1926	13,9	—
- —	1927	8,0	—

Statolithenzuwachs des 4. Lebensjahres:

im Jahre	1925	9,2	Mikrometerteile ...
- —	1926	10,7	—
- —	1927	10,1	—
- —	1928	11,8	—

Als ein weiterer Faktor, der den Fang beeinflusst, möchte ich die Konzentration marktfähiger Schollen auf den Fanggründen nennen. Nach den Untersuchungen des »Poseidon« war die mittlere Zahl der Schollen von 25 cm und mehr in allen Fängen aus der deutschen Bucht 1929 nicht grösser als im Mai 1924. Berücksichtigt man aber nur die Stationen, die wirklich gute Marktfänge brachten und wo Kutterfischerei wirklich ausgeübt wurde, so befanden sich 1924 nur 200 dieser grösseren Schollen im Stundenfang gegen 365 im Jahre 1929. Somit war der Fangbetrieb 1929 in der Tat lohnender, da die brauchbaren Schollen an begrenzten Stellen angehäuft und nicht gleichmässig

Stundenfang von Schollen über 25,0 cm in der Deutschen Bucht. Grundschleppnetz des »Poseidon«.

		alle Stationen	reichste Fangplätze
1924	Zahl	102	201
	Gewicht	18,5 kg	37 kg
1929	Zahl	101	364
	Gewicht	21,8 kg	75 kg

über das Gebiet verstreut waren wie 1924. Die deutschen Kutter landeten im Mai 1924 680 t Schollen gegen 340 im Mai 1924.

So könnte es scheinen, als ob die Kontrolle der Fluktuationen in der Nordsee eine recht undankbare Aufgabe wäre und die darauf zielenden Untersuchungen den Arbeitsaufwand nicht lohnten. Wir wissen jedoch nicht, wohin die Entwicklung des Nordseeschollenbestandes geht, und ob wir nicht in wenigen Jahren vor einer ganz anderen Lage der Dinge stehen. Da nun aber eine Vorhersage des Fanges mit Erfolg erst nach langjährigen Beobachtungen vorgenommen werden kann, so sollte man nicht zögern, die Arbeiten inangriff zu nehmen. Werden doch diese Untersuchungen uns wichtige Aufschlüsse auch über alle anderen Teile des Schollenproblems liefern!

Untersuchungen in anderen Gebieten.

Für die ferner liegenden Gebiete Island und das Barentsmeer ist es natürlich untunlich, ein Programm für laufende Untersuchungen aufzustellen. Diejenigen Länder, die in diesen Gebieten Untersuchungen auszuführen beabsichtigen, könnten aber von Fall zu Fall zur Mitarbeit aufrufen, etwa in dem Sinne, dass gleichzeitig mit Forschungsfahrten Altersuntersuchungen an Marktmaterial vorgenommen würden. Aus der Arbeit von A. Vedel Tåning ist zu entnehmen, dass in der Islandsee intensive Fluktuationen vorkommen, dass aber der Ausfall der Jahrgänge nicht an allen Küsten gleich zu sein braucht. Da ferner die Wachstumsgeschwindigkeit der Schollen je nach den hydrographischen Bedingungen des Ortes wechselt, desgleichen auch das Alter der marktfähigen Schollen sehr verschieden ist, so stehen wir hier vor sehr schwierigen Verhältnissen.

III. Die Ursachen der Fluktuationen und Massnahmen zu ihrer Erforschung.

Mögliche Ursachen der Fluktuationen.

Bevor man daran denken kann, die Ursachen der Fluktuationen zu analysieren, muss man sich darüber Rechenschaft ablegen, wo diese Ursachen liegen können. Da die Fluktuationen bereits auf den ersten Bodenstadien zu erkennen sind, müssen die Ursachen derselben schon auf die pelagischen Stadien wirken. Hier aber gibt es vielerlei Möglichkeiten. Wir vergegenwärtigen sie uns an einer schematischen Übersicht:

A. Menge der produzierten Eier. Diese bedingt:
   I. durch Grösse und Zusammensetzung des Laichbestands.
   II. durch die Laichwilligkeit der Fische (Eimenge von Fischen gleicher Grösse Schwankungen unterworfen, Nichtlaichen eines Teils der schon geschlechtsreifen Fische.

B. Sterblichkeit der Eier und Larven:
   I. Infolge der Bedingungen der unbelebten Umwelt: Überschreitung der extremen Temperaturen, Salz-Konzentrationen, sonstige chemische Einflüsse, Wellenschlag, mechanische Schädigung infolge Absinkens auf den Boden.
   II. infolge der Zehrung durch tierische Feinde und Parasiten.
   III. infolge Nahrungsmangels.

Angesichts einer solchen Fülle von Möglichkeiten, von denen zudem noch keineswegs eine die andere ausschliesst, muss man gestehen, dass eine systematische Untersuchung aller Faktoren gleichzeitig nicht möglich ist. Das richtige Verfahren wird es sein, den Faktor, dem mit Wahrscheinlichkeit der stärkste Einfluss zugeschrieben werden kann, auszuwählen und Untersuchungen zu planen, die erkennen lassen, ob dieser Faktor wirksam ist oder nicht.

Welchen Faktor soll man bei der Scholle für die Untersuchung herausgreifen?

Die Menge der erzeugten Eier ist zweifellos von Einfluss auf die Häufigkeit der daraus entstehenden Brut. Wie auch die nachfolgende Zehrung sein mag, sind viele Eier vorhanden, so haben mehr die Chance zu überleben, als wenn wenige den gleichen Bedingungen ausgesetzt wären. Dass bei einer grösseren Zahl von Larven die Nahrungskonkurrenz zu erhöhter Sterblichkeit führen sollte, vermag man nicht zu glauben, wenn man sich eine (Buchanan-Wollaston) Vorstellung davon macht, wie gross der Lebensraum ist, der jedem Individuum selbst bei grosser Dichte für sich allein zur Verfügung steht. (Die Nahrungskonkurrenz auf den Bodenstadien bleibt hierbei ausser Betracht.) Auch das wenige, was an tatsächlichen Beobachtungen zur Verfügung steht, spricht zugunsten des Einflusses der Eimenge auf die Brutmenge. Buchanan-Wollaston fand 1911 in der flämischen Bucht viel weniger Eier als 1921. Der Jahrgang 1911 war nach Johansen's Untersuchungen einer der spärlichsten im Nordseegebiet. Jahrgang 1921 aber erwies sich teils als reich, teils als sehr reich. In den Jahren nach dem Kriege, 1919—22, wo eine grosse Eiproduktion infolge der Anhäufung laichfähiger Schollen während des Krieges zu erwarten war bzw. nachgewiesen ist, entstanden nacheinander vier teils gute, teils sehr reiche Jahrgänge.

Eine andere Frage aber ist es, ob die Unstätigkeit, das Fluktuieren der Jahrgänge auf die Eiproduktion zurückzuführen ist. In einem Bestand wie dem der Nordsee ist das unwahrscheinlich. Da viele Altersgruppen am Laichen teilnehmen, kann sich die Grösse der Eiproduktion durch Eintritt einer wechselnden Menge von Neulaichern nur langsam ändern, zumal nicht alle Individuen eines Jahrgangs im selben Jahr zum ersten Mal laichen und die Laichproduktion der erstmalig laichenden Individuen noch nicht sehr gross ist. Dass die verschiedene Laichwilligkeit der Fische einen bedeutenden Einfluss auf die Eiproduktion haben könnte, ist gleichfalls nicht wahrscheinlich. — In einem Bestande wie dem der Beltsee und des Kattegats, wo alle älteren Jahrgänge spärlich sind und wahrscheinlich ein wesentlicher Anteil des Laichbestandes von den Neulaichern gestellt wird, ist es schon eher vorstellbar, dass die Eiproduktion in hohem Masse schwankt und die Fluktuationen zwar nicht verursacht, aber vielleicht verstärkt.

Die unmittelbare Wirkung der Bedingungen der leblosen Umwelt auf das Überleben der Eier kann in weiten Gebieten nur minimal sein. In der Nordsee z. B. kommen Salzgehalte, die so gering sind, dass die Eier sich ihnen nicht anpassen könnten, nicht vor. Wo sie beobachtet werden — in der Ostsee — da bewirken sie ein Absinken der Eier (Johansen), aber dadurch gelangen die meisten von ihnen, wie Johansen ausführt, in die tieferen salzhaltigen Schichten, in denen sie wieder schwebfähig sind. Apstein hat nachgewiesen, dass die Eier der Scholle auch bei Temperaturen unter 0° C. entwicklungsfähig sind, und so geringe Temperaturen können überhaupt nur in einem sehr

kleinen Teil der ganzen Wassermasse jemals beobachtet werden. Ferner weist Buchanan-Wollaston darauf hin, dass sich die Scholleneier schnell in der ganzen Wassersäule verteilen. Nur ein kleiner Teil also befindet sich in den oberflächlichen Schichten, in denen eine Schädigung des Embryos durch den Wellenschlag denkbar ist (Rollefsen).

Für das südliche Kattegat und die Beltsee besteht wie Johansen nachgewiesen hat, eine unverkennbare Korrelation zwischen dem Ausfall der Jahrgänge einerseits und der Wassertemperatur und -Salinität (Oberfläche bei Anholt-Knob) in den ersten drei Monaten des Jahres, also der Zeit der pelagischen Entwicklung der Brut. Aber Johansen nimmt eine indirekte Wirkung der hydrographischen Verhältnisse an, die unmittelbar nur die belebte Umwelt der Larven beeinflussen sollen. Zwei Möglichkeiten haben wir hier ins Auge zu fassen, die Schwankung der Zehrung durch tierische Feinde, die in wechselnder Menge auftreten, oder Schwankung der Zehrung durch Hunger infolge wechselnder Ernährungsbedingungen. Johansen entscheidet sich für die zweite Möglichkeit und begründet das dadurch, dass zwischen dem Gesamtvolumen des Planktons in den ersten drei Monaten des Jahres und dem Ausfall der Jahrgänge für einige Jahre eine sehr scharfe Korrelation bestanden hat. Allgemein ist in der Beltsee im Frühjahr wärmeres und salzhaltigeres Wasser reicher an Plankton als kaltes und schwachsalziges. So ist es anzunehmen, dass die Schollenlarven in Jahren hoher Temperatur und hohen Salzgehalts gute Ernährungsbedingungen finden. In den Jahren, wo die entgegengesetzten Verhältnisse herrschen und wo arme Jahrgänge entstehen, sind aber mit der Gesamtplanktonmenge auch die tierischen Feinde der Larven — vor allem kommen hier Medusen infrage — spärlich.

Blegvad's Untersuchungen zeigen, dass in der Ostsee wenigstens in den letzten Jahren die gleichen Jahrgänge gut bezw. schlecht ausgefallen sind wie in der Beltsee. Wenn das allgemein zutrifft, so wird man geneigt sein, die Identität der wirkenden Ursache anzunehmen. Gleichartig auf beide genannten Gebiete wirkt aber nur die Temperatur, nicht der Salzgehalt. Das ist sehr wohl vorstellbar: In kalten Wintern wird die Frühjahrswucherung des Planktons besonders lange hintangehalten, sodass die Larven in diesem Falle besonders wenig Nahrung finden. — Andererseits gibt Johansen selbst die Erklärung dafür, dass auch eine Korrelation zwischen Salzgehalt und Jahrgangsstärke besteht: Salinität und Temperatur stehen untereinander in scharfer Korrelation, weil in kalten Wintern der vorherrschende Ostwind besonders viel salzarmes Ostseewasser in die Beltsee hinaustreibt. Andererseits lassen Johansen's neueste Untersuchungen die Möglichkeit offen, dass in Jahren starken Einstroms salzreichen Wassers von Norden in die Beltsee Schollenbrut in grosser Menge passiv eingeführt wird. Diese Zufuhr könnte die Fluktuationen in der Beltsee wonicht ausschlaggebend, so doch merklich beeinflussen.

Für andere Gebiete fehlt noch jeder Anhalt dafür, worin die Ursachen der Fluktuationen liegen, aber es erscheint logisch am richtigsten, wenn man hier zunächst prüft, ob die gleichen Ursachen wie in der Beltsee wirksam sein können, also die Nahrungsmenge. Auf Grund der bisherigen Untersuchungen können wir nur sagen, dass das nicht undenkbar ist.

Der wichtigste Laichplatz für die Scholle liegt in der flämischen Bucht, und zwar,

wie besonders klar von Buchanan-Wollaston dargetan ist, in dem relativ warmen und salzhaltigen, vom Kanal her eindringenden Wasser. Dieses Wasser aber ist nach Wulff und van Goor besonders arm an Plankton, sowohl an Diatomeen wie an kleineren Formen. Vielleicht hängt das Überleben der Brut davon ab, dass sie durch den Reststrom rechtzeitig in die Gegenden gelangt, wo sich das Kanalwasser mit dem nach Wulff ungleich planktonreicheren Küstenwasser mischt.

Hier gewinnen nun die englischen Untersuchungen über den Reststrom in der Kanalmündung und der flämischen Bucht eine ausserordentliche Bedeutung. Es sei darauf hingewiesen, dass der normalerweise etwa N.O. setzende und im Winter besonders kräftige Strom, der die Larven an die Kontinentalküste führen würde, an der Oberfläche und z.T. auch über dem Boden durch den Wind beeinflusst, ja durch stürmische N. und O.-Winde sogar umgekehrt werden kann. Es wäre daher garnicht undenkbar, dass die Wetter- und Stromverhältnisse auf diesem indirekten Wege den Ausfall der Jahrgänge beeinflussten. Einen ähnlichen Gedankengang deutet Thursby-Pelham 1928 an. In diesem Zusammenhange darf vielleicht erwähnt werden, dass sich bei oberflächlicher Prüfung der Wetterverhältnisse ergab, dass im Januar—Februar 1924 ungünstige, im Januar—Februar 1925 und 1928 günstige Verhältnisse herrschten: 1924 wehten über dem Kanal teils südöstliche, teils nördliche Winde, und über der flämischen Bucht scheinen besonders im Februar Nordwinde geweht zu haben. Jahrgang 1924 war schlecht. 1928 aber war der Wind in Cashot W.S.W., bei Calais und Vlissingen S.W., bei Helder wieder W.S.W., und zwar sehr stark. Jahrgang 1928 verspricht sehr gut zu werden.

Ein Zusammenhang ist nicht undenkbar, aber wie steht es mit der wirklichen Kenntnis der Lebensverhältnisse der Larven?

Die Angaben über die lokale Verbreitung der Schollenlarven nach dem Ausschlüpfen, ihre Trift, Ort und Zeit des Übergangs zum Bodenstadium sind nur sehr allgemeiner Natur, nicht geeignet zur Formung eines vollständigen Bildes. Über die Veränderlichkeit dieser Vorgänge ist kaum etwas bekannt. Es gibt in der Nordsee ferner mehrere wohlunterschiedene Laichplätze. Es ist aber nicht bekannt, welchen Küstenabschnitten die dort erzeugte Brut zugeführt wird. Es ist ferner nicht klar, ob die Bedingungen, die den Ausfall der Jahrgänge beherrschen, auf mehreren dieser Laichzentren gleichförmig wirken. Daher bin ich der Ansicht, dass die Analyse der Fluktuationsursachen genaue Untersuchungen über das Leben der pelagischen Larven zur Voraussetzung hat. Sollen solche Untersuchungen z.B. in der Nordsee aufgenommen werden, so müsste man die folgenden Arbeiten empfehlen:

Trift und Verbreitung der Scholleneier und -Larven von den wichtigsten Laichplätzen aus ist mehrere Jahre hindurch zu verfolgen. Quantitative Proben oder, wo das wegen der Seltenheit und Beweglichkeit der Larven nicht angängig ist, möglichst vergleichbare Fänge mit Horizontalnetzen müssen hierzu während der Entwicklungszeit mehrmals über grösseren Arealen ausgeführt werden.

Bei den gefangenen Larven aller Stadien ist der Inhalt des Verdauungstraktus zu untersuchen, um zuverlässigen Anhalt über die Art der Nahrung zu gewinnen.

Um sich über Verbreitung und Häufigkeit der Nahrungsorganismen zu unter

richten, müssten auf den Untersuchungsstationen Proben des Planktons mit geeigneten, möglichst quantitativen Geräten genommen werden.

Durch derartige Untersuchungen, die allerdings mühsam und zeitraubend sind, würde unsere Kenntnis des Meereshaushalts in wertvoller Weise bereichert werden.

Summary.

I. Methods.

A study of the relative age-composition of the stock is not sufficient for an accurate determination of the fluctuations of the plaice, and still less so for a determination of the numerical strength of the single year-classes. What is necessary is a determination of the density — i.e. fish caught per unit of fishing.

A determination of the density of the first bottom-stages, according to JOHANSEN's method, gives a good idea of the strength of the single year-classes where extensive investigations have been made. But a standardisation of this method can be recommended only where the coasts are smooth and have a narrow littoral. For rugged coasts with strong tide-currents, such as on the German low coasts, the investigations would necessarily be too extensive.

Accordingly, as regards the North Sea, a determination of the density of the year-classes of plaice is recommended to be made by investigating the 2, 3 and 4 years old fish in deeper water. In order to limit the area of operations, investigations should, as far as possible, be made at a season when the younger plaice are concentrated at a narrow space near the coast (in spring). Market statistics are not adequate for density determinations. The density must be investigated by as many catches as possible from research steamers, and the net of stations at which catches are made must be as regular as possible.

The coefficient of consumption can be calculated through the decline of the yearly average catch of a year-class from one year to the other. To this end, the English statistics can be used as far as the older groups are concerned, since the English plaice fishery is carried on fairly regularly both in shallow and deep grounds.

II. International co-operation for making forecasts of future yields.

In such areas as the North Sea where many age-groups contribute considerable quantities to the catches, the productiveness of a single year-class can only be of minor importance for the total yield. The influence of the fluctuations upon the yield may become clearly perceptible in areas where the catch relies upon a few age-groups only.

In order to prepare forecasts of future yields two kinds of investigations must be made:—

1) Determination of the productiveness of the year-classes. Determinations of density made in the North Sea on 2 to 4 years old fish, in the Kattegat and the Baltic on 1 and 2 year old fish and in the Belt Sea on the 0-group.

2) Control of the age-composition of the fish landed in order to ascertain the average share of each age-group and the influence of the strength of the single year-classes upon the yield.

Further, a control of the rapidity of growth is necessary, as this rapidity determines the time when a year-class can be utilised by the fishery. The intensity of fishing too must be taken into account.

III. Causes of Fluctuations.

In the Belt Sea, the productiveness of the year-classes is probably determined by the amount of food available for the larvae. The researches in other areas ought in the first instance aim at elucidating the question whether also there a varying amount of food is the cause of the fluctuations.

To this end are recommended simultaneous researches relating to local distribution, to the frequency and drift of the larvae, to the stomach contents of the larvae, and to the distribution and frequency of the food organisms. These researches ought to be continued for several years and ought to be made on an extensive scale, both as regards time and space.

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DISCUSSION

1. Dr. **J. N. Carruthers** expressed particular interest in the following consideration regarding the Plaice:—

If, at the time of spawning and shortly thereafter, the water movements in the neighbourhood of the important spawning areas chance to be markedly different as between one year and another, one may reasonably expect some sort of reflection of this physical fact to be manifest a few years afterwards in point of year-class strength.

In conversation, he (Dr. Carruthers) had learnt from the Heligoland workers, that unusually large catches of postlarval plaice had been made recently by them, and it was certainly an interesting fact that, throughout the entire time span of the most recent Plaice spawning in the Southern Bight, there had been an overall water flow from Channel to North Sea some $2^1/_2$ times as great as occurred during any of the three previous corresponding seasons. One could hardly imagine current conditions more likely to secure the transport of the entire spawning products to those nursery grounds whose attainment is held to be of such importance — than prevailed during this last spawning season.

2. Prof. **E. van Everdingen** was much pleased to note, that the Council was approaching the solution of the problem of utilising hydrographic observations in explaining and predicting fluctuations in the abundance of fish. With reference to Dr. Carruther's remarks, he should like to know the time-accuracy obtained with the new English drift bottles. He further pointed to the possibility of utilising a large material of current estimates on board Dutch light vessels, collected at his office at De Bilt, but not yet worked out for want of time, in the interest of these researches, as this material was complete and continuous, excepting stormy periods, before and after the war.

IV. HERRING

Fluctuations in the Stock of Fish.

Herring.

By

Einar Lea.

A. Introductory Remarks.

The eight papers concerned with fluctuations in herring tribes or populations read at one of the special meetings, arranged in London last year, include experiences from areas characterised by widely different external conditions, and the tribes or populations, from which the material of observations has been drawn, exhibit great differences in many respects, in rate of growth and physiological development generally, in the longevity of the generations, in the extension of the area of distribution and in the magnitude of the tribe or stock.

These great diversities in internal constitution and external conditions constitute a warning against generalisations, if not founded upon special facts.

But in all these different circumstances, under which the herring may live, and with all differences in the character and life-history of the different tribes, fluctuations have been observed which cannot be due to defective methods and must be considered as more or less correct reflections of states and happenings in nature, phenomena which will play their part in the researches aiming at a better understanding of the natural basis of the fisheries.

And thus it comes that rational investigations of the fluctuations, their causes and their consequences ought to become an important part of the international co-operative investigations.

When plans for international cooperation in this field of research are to be contemplated for the fluctuations in the tribes of herring, our knowledge of the general features in the biology of our species ought to be kept in mind. Owing to the shoaling and migratory habits of this fish, and owing to the existence of races or tribes with partly overlapping areas of distribution, it seems necessary to combine the special study of fluctuations with researches aiming at an elucidation of the race-problems. There is so much more reason to do so, as the race-problems cannot be considered as sufficiently solved even for such an important area as the North Sea and adjacent waters.

Secondly it is very desirable to arrive at some agreement as to a certain measure of uniformity of technique in the execution and notation of the basic observations. As

an instance may be mentioned the fact that the total length of the herring is at present measured according to at least two different units of what must be considered the total length and that the lengths thus measured are arranged in two different ways (between different group-limits) the same descriptive terms being used for both sorts of grouping.

In view of this state of things as much as from the experience that the centimeter grouping is for certain purposes too inexact, it is proposed that a system with half-centimeter grouping be adopted for future work. By this means, which will involve only a little more labour, frequency curves may be constructed from future observations and be strictly comparable with those from earlier observations in either of the two systems.

A standardisation is also needed for the terms year-class, age-class etc., for the terms used in growth-calculations and for the biometric characters.

B. Validity of the Methods in Use.

The results described in the papers under discussion suffice to show that it has been possible, by means of comparatively few observations, to arrive at fairly definite conceptions concerning the age-composition of the group of mature herring belonging to different herring tribes.

That this should prove to be possible was by no means self-evident, even in those cases, where the extended use of non-selective fishing gear offered the best opportunities for the work of procuring representative samples for analysis, as in the Norwegian spring fishery.

Still more astonishing is the experience that even the analysis of samples from catches made with drift-nets, known to act selectively, have lead to similar results, as is shown for instance in the fig. 1 of HODGSON's paper, where it can be seen that characteristic features in the observed age-composition repeat themselves in the observations for the next year or years. This applies especially to the older year-classes, where in HODGSON's words "the proportion which one bears to another is similar for several years, that is, the general outline of the figure remains constant, suggesting that the rate of mortality is acting equally on all the groups of fish", a conclusion which is identical with that drawn from the observations on the Norwegian herring tribe, where the samples have been collected from catches made with non-selective gear.

Even if it cannot be concluded from HODGSON's observations that the proportionality in the young year-classes is equally representative for the conditions in nature, his experiences are still extremely important in this connection by removing some of the doubt as to the representative value of net-caught samples. The problem of how the net-selection contributes to the difference between actual and observed age-composition is an important one. It has been treated by HODGSON and BUCHANAN WOLLASTON and deserves to be further studied by means of experimental fishing and by investigations with growth-calculations as material.

The most important task of procuring adequate observation material for a season's fishery on a tribe or stock of herring depends for its solution on several circumstances which may differ in different cases. The exactitude wanted in the results and the sum

of labour and money to be spent are the two (antagonistic) factors involved. The opportunities for procuring samples, dependent on the fishing operations may likewise be of importance. The constitution of the stock of herring to be explored, its stability with regard to age-composition, its extension in space and in the period of occurrence must also be considered.

It is, therefore, useless attempting to lay down definite rules for sampling, schematically applicable in the different cases. But from the experience hitherto gained some hints for the proper execution of the sampling may be given. The investigation of a congregation of grown herring in the spawning region is taken as an example.

In such a case it is most important to secure samples from the very commencement of the fishery. These samples may often prove to be of the utmost value for the critical examination of the whole season's material. Something similar applies, according to the experiences made in Norway, to the last samples of the season. And it is, I think, a good proposition to let these "end-samples" be rather larger than those between, as they will stand so to speak unsupported by other samples on one side.

Another very important period for collecting samples occurs when the spawning of the herring is at its height. During this period the great mass of material ought to be collected for the purpose of obtaining the most valuable observations for establishing the age-composition of the classes of older herring.

Between the "end samples" and these "central samples" some "interpolating samples" ought to be placed in order to form connecting links. The geography of the fishery must also be considered during the work. Samples from the boundaries of the fishery may contribute to the understanding of the whole series of observations and to the feeling of confidence which ought to result from the subsequent critical examination of the material as a whole.

In these considerations I find myself in essential agreement with M. LE GALL, who has in the introduction of his very interesting paper treated this question of sampling, and it is according to these principles one has tried in Norway for many years now (since 1914) to secure an adequate material for exploring the great spring fishery there.

But I cannot go so far as he goes in the question of the number of individuals in each sample. It is desirable to be able to operate with larger samples, partly to minimise the effects of personal or other bias in the samples, but chiefly with the view to facilitating the critical examination of the whole system of samples for a season. One test of adequacy of sampling and consistency of results is to make sure, whether each particular sample is connected in a rational way with its neighbour-samples (in time and space) and whether it will fit in as an intelligible detail of the entire picture formed by the whole system of samples. For this reason each particular sample ought to be large enough to show the characteristic details in the age-composition and not only the broad features.

When next the average age-composition for the season is to be worked out by a combination of all the single samples it has proved necessary, at least for the Norwegian material, to resort to a statistical weighting of the samples which is not dependent on the number of individuals in each sample, but on the approximate number of herring

caught and for which the sample is a representative[1]). And it has repeatedly occurred that a considerable weight had to be conferred upon one or more particular samples. If the chance errors of these had been great, as they will be in small samples, the benefits of the weighting would have been counteracted by the undue introduction of greater chance errors of sampling into the final results of the statistical treatment.

It may be argued that such a dilemma will arise only when errors are committed in the distribution of the samples over the "field of fishery", and this may be true. But on the other hand such errors are really unavoidable when the work is to be carried out within a short interval of time and within stated limits of labour and expense.

For these reasons the number of individuals in each sample ought to be high enough for the sample to "stand alone", if such an expression is permitted.

For the Norwegian tribe of herring, samples comprising 200 to 300 individuals seem to be a reasonable makeshift between the two antagonistic demands: many samples and many individuals in each sample. For other tribes, where the number of year-classes is not so great as in the Norwegian tribe, a number of not less than 100 individuals per sample seems desirable.

A further and most important test of the adequacy of sampling and of consistency of results is the comparison of the average age-composition for one season with those of the former years. In this way some cases of inconsistency can be detected even in the small-scale fig. 1 of my paper, intended more to give a general outlook than to be submitted to a close scrutiny of details in the age-composition of the group of Norwegian spawning herring. If the numerical data had been arranged in a more convenient manner for this purpose (see fig. 1 of Dr. Andersson's paper) these inconsistencies would have been more apparent.

The possibility of determining mortality rates for the different tribes of herring, or rather the amount of work and the number of years required for arriving at definite conceptions as to the numerical values expressive of mortality, will depend upon several circumstances which will differ from tribe to tribe. In a tribe, wherein no year-class fluctuations occur, the age-composition, if correctly observed, would take the same shape monotonously from year to year. Some few years observations on such a tribe would suffice to reveal this fact and to establish, by means of simple calculations, the desired numerical expressions for the mortality rates.

If any real tribe of herring approaches this ideal, the desired results may be reached after a rather short lapse of years. But as all the papers under discussion here are so many documents illustrative of rather marked fluctuations, it is to be feared that a regular observation-service, patiently continued year after year, will be required in order to arrive, in due time, at the desired result.

However, no hesitation need be intertained in adopting such a course of work. For the observations accumulated for this purpose will, when properly combined with other data (length, weight, growth etc.) necessarily become a fund of basic observations

[1]) The principles of this weighting are stated in my contribution to Hjort's paper in Rapports and Procès-Verbaux XLI (September 1926).

of the utmost importance for the elucidation of the general biology of our species. And a two years period of very extended sampling for the purpose of establishing the form of the "rule of mortality" in different tribes or stocks will, in my opinion, constitute a proper beginning of this work.

C. Organisation of Regular Cooperative Studies on Fluctuations.

For any kind of cooperative research to offer good prospects of success it is essential that some first conditions are fulfilled: that the participants are in agreement with regard to the methods of observation to be used, that these methods are in themselves really sound, that the observations made by the individual workers are of a high degree of comparability, free from serious personal bias, and finally, in the case before us, that there are not serious gaps uncovered by observations in the field of research.

Therefore it is necessary, before attempting to formulate plans for cooperative work, to try, on the basis of experience already at hand, to form an opinion on the present state of affairs in these respects.

I believe I am on the right side, when I state as my opinion that for all observational details, such as the manner of taking length-measurements, of counting the number of vertebrae etc. all that is necessary in an agreement in order to secure the desirable standardisation and comparability of results, and that the situation is somewhat more complicated, when the question of sampling, the method of growth-calculation and the method of age-assessment are considered.

With regard to the question of sampling I have already stated my views.

Concerning the growth-calculation method it must be said at first that the use Hodgson has made of it in his paper is a good example for showing its importance for clearing up the intricate modes of recruitment of the shoals constituting the great East Anglian herring fishery by individuals originating from different spawning seasons and areas. Next it must be pointed out that this method will most certainly prove to be still more important for the study of fluctuations and the renewal of the stock, when the approximate formula commonly used has been replaced by one giving correct average results, so that the arteficial part (caused by the use of the approximate formula) of what has been termed the phenomenon of apparent change in growth rate is removed, whereby the pure, biological part of the phenomenon can be turned to account. The phenomenon consists in the fact that the average values of corresponding calculated growth-dimensions are rather consistently greater for younger fish than for older ones, and in the case of material from shoals of grown herring it is an expression for the fact that sexual maturity and inclusion in the shoals of spawners of the young herring is attained earlier by the well-grown members of any particular year-class than by those individuals, whose growth-history has been less favourable in this respect. For this reason it will be possible to use the growth-data for the purpose of following, and possibly of measuring, the most important process of the successive inclusion in the spawning-stock of the young recruit-spawners. As this process is of great importance at the same time as it is difficult to follow by means of age-observations alone, this other method of

studying it should not be neglected, and it is therefore reasonable to include systematic work for the purpose of improwing the growth-method in the prògramme for researches into the fluctuations.

Something similar applies to the method of age-assessment by means of counting the annual zones visible on the scales and in the otoliths. More uniformity and comparability is needed. And, judging from the work done in Norway, this high standard is not easily attained. The difficulties are deeply seated in the method itself, as the signs impressed by nature into the scales or otoliths are not always equally readable and will in some cases be differently deciphered by different observers and even by the same observer by independent repetition.

However, in spite of this peculiarity of the age-method, it has been a source of satisfaction to observe, by reading the papers under discussion, that the observations of all (Dr. JOHANSEN'S paper excepted, as it is not concerned with age-reading) exhibit features pointing to the conclusion that the great mass of age-readings have been correct. The similarities of results in the papers concerned with the herring in the North Sea and the English Channel are proof of this. And at the same time they ought to provoke a strong wish to have this matter further investigated by means of more material, treated in so uniform a way as possible. There is here a rather clear case of one of the problems mentioned in Dr. RUSSELL'S programme for the discussion: the relation between fluctuations in the same species in different areas. Dr. ANDERSSON'S very interesting remarks on the connection between the fluctuations in the Baltic and the Norwegian herring is another case.

In order to attain a high standard of uniformity in observations of this kind, it is, I believe, necessary that the several observers by some means are enabled to get an intimate knowledge, characteristic samples of each others material and of their ways of reading in clear as well as in doubtful cases.

The desirability of avoiding serious gaps in a given field of research is quite obvious. But it is not quite so easy to tell how this may be accomplished in such cases, where one or more countries, for their own good reasons, are not prepared to participate to the desirable extent, in areas frequented mainly or exclusively by their own fishermen. There are, possibly, two ways to avoid this obstacle, provided the non-participating countries will be able to offer some assistance in procuring samples.

Either an arrangement with another country, preferably a neighbour-country, could be brought about concerning the further treatment of the samples, or a sort of international service-central could be established for the purpose of treating raw-material as far as desired — by the institutions interested. It is obvious that such a service-central, if it were realised, could in addition contribute very much towards the standardisation of the observational work and perhaps it would prove to be the most economical means of placing the cooperative work on a sound basis.

The idea of such a service-central is now rather old, as it was suggested by Professor HJORT, then Director of Fisheries for Norway in the years 1909—10. But it was launched, as can now be seen, at too early a moment.

So far nothing has been said about the problem of making reliable forecasts of

future yields of the fisheries and of throwing light upon the relations between fluctuations in different species of fish.

To begin with the last mentioned question, it seems, in advance, reasonable to assume that such relations exist between fluctuations in the herring tribes and fluctuations in other species of fish. There is even a special reason for operating with such an assumption in the case of the herring. For this fish species stands alone among the important saltwater food-fishes in being a demersal fish, depositing its eggs on the bottom without doing anything to protect them during the period of incubation.

We know further, principally from the Scottish observations, that the haddock and other gadoid fishes are greedy consumers of herring roe, and the Norwegian observations on herring roe dredged from the bottom have raised the suspicion in me that the roe-eaters, besides being gready, are rather fastidious in their tastes and skim off the uppermost and living eggs from the thick flakes deposited, leaving behind the dead eggs underneath.

A rich stock of roe-eating fish can, I suppose, do great damage when present on the areas covered with herring roe. And from the results of the cod- and coalfish-fisheries, carried on during the incubation period of the herring roe in the Norwegian spawning areas, it may be assumed that the stock of roe-eaters varies from year to year.

But our knowledge along these lines is for the present scanty and incoordinate, and it is to be hoped that this problem will come within the range of more systematic observation, when the fluctuations in more species of fish have been studied during a longer period of years.

Concerning the problem of forecasting the yields of herring fisheries it must at first be pointed out that the yield of a seasons fishery in a particular field is bound up in causal connection with several factors, among which the size of the stock is one. Dr. Lissner has emphasised this and mentioned the nourishment, the growth and the weather conditions as such factors. The hydrographical condition and its variations in the field of fishery were the subject of very interesting studies by the late Dr. Gustaf Ekman, Professor Otto Pettersson and Professor Hjort many years ago, and even cosmophysical events have been, and may still be considered as causal factors.

Whatever importance be assigned to anyone of these factors, it seems to be clear from experience already gained that the year-class fluctuations with their effects on the size of the stock of herring is in fact only one of several contributory factors, a factor which may be found to be of more importance in some cases than in others.

Dr. Andersson has, I believe, made out a good case for the assumption that year-class fluctuations are of comparatively great importance for the yield of the fisheries in the Baltic. In the case of the fisheries based upon the mature Norwegian herring the situation is rather different. And the explanation of this difference will probably be found in the considerable differences in the rate of mortality in the two stocks and in a difference in the intensity of the fishing of the two stocks.

Mr. Aage J. C. Jensen has produced some evidence in favour of the view that the quantity of spawning herring in the southern Baltic and the Sound has a considerable influence upon the size of the stock in some of the following years. His results, together

with Dr. Andersson's and those of Dr. Johansen concerning the correlation between the catch of spawning herring and the catch of herring larvae seem to be favourable for the assumption that for the Baltic and the Sound herring some rather direct measure for the future size of the stock and the yield of the fisheries may be found when the investigations have been carried on for some time.

Through a correlating of data in the fishery statistics for Norway with the results of the bio-statistical researches it can be seen that the years of maximal yield of small-herring and young herring correspond with the occurrence of rich year-classes. The 1925-class gave a record fishery of small herring during the summer and autumn of 1926, and resulted in an immense fishery of young herring in the autumn of 1928 and the first months of 1929. The year-class 1918 corresponds with a record yield of small herring in 1919 and the 1913-class to a record yield of small herring in the autumn of 1914.

The 1904-class, the best of all, did not occasion a rich fishery in 1905 and 1906. But at that time the commercial demand for small herring (as raw-material for the production of herring oil and meal) was very small. But during the years 1907—1909 this year-class gave a large yield of young-herring. The rich year-classes in the Norwegian tribe thus have made themselves known by an unusual success of the fisheries for small herring. But also in some other ways hints for a prediction of the amount of the renewal of the stock of spawners are given. The invariably small percentage of three year old spawners is an indication of the numerical strength of the year-class, to which they belong. They act as herolds for the later arrival of the main body of the year-class. And there is also some evidence for the assumption that a rich year-class, when the individuals are in the youngfish stage, will spread themselves over a wider area and will be found by the fishermen in unusual places.

For tribes of herring which are not pursued by the fishermen until maturity is reached, some sort of standardised experimental fishing for the young herring approaching maturity might, perhaps, give valuable clues.

D. Investigations of the Causes of Fluctuations.

There are very good reasons for the assumption made by Professor Hjort and others that the fate of a year's brood of herring and other fishes with respect to its numerical strength is decided during the early stages of its life.

And there are also definite facts pointing to the conclusion that the number of eggs produced is not a principal cause for the fluctuations, although for some tribes of herring it may be, as Dr. Johansen says, a factor we dare not omit to take into consideration.

There is further, I assume, a consensus of opinion in favour of the hypothesis, propounded by Professor Hjort that this determination occurs, when the yolk carried by the newly hatched fry is being consumed and extraneous nourishment must be sought for by the fish fry. The comparatively easy artificial hatching of fish eggs with the subsequent difficulty of rearing the fry constitute a good circumstantial evidence for this assumption, and it seems natural to take it as the main working hypothesis for plans

of researches aiming at a solution of the problem of the causes of the fluctuations in the annual broods of herring, although other possible causes ought not to be left out of sight.

If this proposition be accepted, it seems reasonable to search for a verification of the nearest possible causal nexus, that of the simultaneous presence in the same masses of water of newly hatched herring larvae and suitable food for them, as opposed to investigations concerning the possible connection between fluctuations in the annual broods of fish and, say, the quantities of inorganic nutritients dissolved in the sea-water.

Investigations along this line need not necessarily be of a strictly quantitative character with regard to the absolute catch of the plankton nets. If they can be made so, so much the better of course, because other aspects of the problem could then be considered. But much could be gained if the proportional quantity of herring larvae in need of food and the quantity of suitable food surrounding the larvae could be established by means of plankton hauls made at the right places relative to the spawning places and at the correct time relative to the hatching period.

In order to satisfy the two last-mentioned conditions it would presumably be necessary to combine the plankton investigations with investigations of the spawning and hatching processes at the actual spawning places, possibly also with hydrographic observations. By such a combination a set of observations bearing upon other contributory causes would be obtained.

The combined activities of two or more exploring vessels working in one selected area before and during the supposed critical period, at least during the first years of work, would contribute greatly to the prospects of success. And as the results of one year's work ought to be available before the work of the next year commences, the idea of a service-central again suggests itself.

V. SPRAT

Methods of Investigation into the Fluctuations in Sprat Stocks.

By

Paul Bjerkan.

Sprat investigations on biostatistical lines have hitherto been carried on mostly in Norway. The obvious reason for this is that in no other country is the sprat utilized to such an extent as in Norway. In the last year, however, England took up sprat investigation on a larger scale and this may be of much interest as many questions regarding the biology of the sprat have local bearings and may best be studied by workers with a good knowledge of the local conditions. From cursory samplings of sprats in other countries material has kindly been sent to Norway for examination, but precisely in the case of the sprat such samplings ought to be undertaken according to special methods if they are to give quite satisfactory results. This will be explained further on in connection with the methods used in Norway.

Owing to the fact stated above, these remarks about the methods used in sprat investigations must necessarily be a description of the methods in vogue in Norway and of a successive adjustment of those methods to the conditions in other countries. A collaboration by those members of the audience, who are especially interested in sprat investigation in other areas is therefore requested, to aid in a proper adjustment to the conditions found in other countries.

The most striking feature in the Norwegian sprat stock is the fact of its being a young fish stock, consisting, for the commercial fishery, mostly of one year old sprats. The investigations are thus limited chiefly to studies of the sprat during its two first summers. It is owing to this that in the principal sprat fishing area of Norway, the west coast, the larger part of the stock is not indigeneous, but has its origin in the spawning districts proper to the sprat, the North Sea and Skagerak coasts. The investigations in this area are thus largely confined to a study of the immigration of the young sprat, while the spawning of the sprat and the mortality questions must be studied in other areas.

As may be understood, the fact stated above restricts the investigations considerably. To a certain extent it facilitates the investigations viz. regarding age readings etc. but in other ways it makes it difficult to follow the movements of the stock, as the year-classes change from summer to summer, and no stock representative of the sprat population, as a whole, is at any time found in the area.

You may from these introductory remarks understand, why I have asked my colleagues to assist in the adjustment of the methods of sprat investigations used for the Norwegian sprat stock, when I have ventured to bring them to the notice of this audience.

In the following I will give a brief description of the methods used in the investigations:—

Sampling. The samples are taken mostly from commercial catches. The number of sprats in a sample is usually about 200. During highsummer, when as a rule only one year-class is represented, as low a number as 100 might be sufficient for samples, which are only measured and weighed. Scale samples ought, even then to be larger owing to the circumstance that many sprats have lost their scales.

The samples are taken at random from the cases. If by a cursory inspection a difference in the size is found a lot is taken from several cases in order to smooth the result. Such conditions are mostly caused by the fact that the larger and the smaller sprat does not always go at the same levels in the seines.

Scale samples. Owing to the fact that sprats are more apt to loose their scales than the herring, not all samples taken from commercial catches can be utilized for scale readings. Many samples give only a few scales, which can be used as a control. Then in all samples the smaller sprats are more delicate than the larger ones and are thus more apt to loose their scales. Below 9 cm. very few scales are found even in comparatively good scale samples taken from commercial catches. The scale readings are thus used only as an auxiliary method; the basis of the studies of the composition of the stock is the measurements, which in all cases agree with the composition found in the catches.

The scales are taken at once and laid out in fresh condition on the slides, as many as 20 being fastened with albumen-glycerin on each slide, in two rows, but always in such a way that every scale may be identified on later inspection if it should be necessary to check some of the readings. As the sprat scales, especially of the smaller ones, are very delicate, it is essential to lay the scales out fresh to get good samples. This is one of the reasons, why samples from other countries, preserved in formalin gave unsatisfactory results, the other reason was that there were generally no scales at all to be found.

If sprat samples should be sent for examination over a long distance I would therefore, from my experience, advise, that the scales be taken off and laid out beforehand if satisfactory results are desired. As in most cases the age readings of the scales are used as an auxiliary method in establishing the composition of the catches, as stated above, no numbering of the sprats etc. is necessary on that account. The scales on the slides are as a matter of course arranged and numbered according to the size of the sprats. The scale samples are thus used as an auxiliary to control the measuring of samples and independently for investigations on growth questions if such are required.

Measurement. The sprats from 3.5 cm. and upwards are measured in half centimeters in such a way that the length used as a group term lies in the middle of the group. Thus group 9 cm. ranges from 8.75—9.25 cm., 9.5 cm. from 9.25—9.75 cm. and so on. This is attained by adding 0.25 cm. to the zero point of the measuring rule, in

conformity with the procedure in vogue in the Norwegian herring and cod investigations. The measurements are afterwards checked by the age readings, where such have been taken. As generally only one — very seldom as many as three — year-class is found the results arrived at by this procedure are quite satisfactory.

Here I may allow myself a digression from the theme of the sprat. Measurements of fish for biostatistical purposes are now made in at least two different ways. If measurements in centimeters are required, some investigators name the group between say: 10 and 11 cm. "10 cm." and others "10.5 cm.". Both expressions give the same measure but the latter is the more adequate term.

By adding half a centimeter (by measurements in cm.) as the Norwegian investigators do, the actual length in whole centimeters lies in the middle of the range of a group. The group "10 cm." then ranges from 9.5—10.5 cm. and so on, and the expression used is quite adequate, when we bear in mind that by the procedure we have transformed the "length measuring rule" into a „group sorting instrument". The difficulty with "limit cases" we still have, this can, however, not be avoided in grouping natural objects.

I would call the attention of the audience to the fact that the two procedures cannot be compared with full accuracy and that the conditions may often cause confusion.

Returning again to the theme of the sprat.

Weighing. The specimens of each length group are weighed as a whole and the average weight calculated. From the length weight relation a quality-indicator for each sample is calculated on the formula:—

$$i = \frac{1000\,p}{l^3},$$

where p and l are the average weight and length respectively.

For the commercial sprat this indicator has most generally been found to amount to a little more than 7.0 during the spring, during highsummer to above 8.0 and in the autumn to between 7.5 and 8.0. Only perfectly fresh samples in good condition are taken into consideration in the calculation with the indicator. As the commercial catches contain only sprat at the same biological stage viz.: immature sprats in the fat stage, such an indicator has its advantage in determining the quality of the sprat.

Fat analyses of the sprat, after Bull's new method have also been taken of a lot of samples, but the quality-indicator seems, at least at the present state of the investigation, to be the better working principle.

Growth calculation. The scale samples and age readings are, as stated, used as a control by establishing an understanding of the composition of the catches. As an independent unit in the investigations, scale samples are, however, the means of studying the growth of the sprat. This is attained by measuring the annual growth zones of the scales and calculating the growth of the sprat thereby in the same manner as used for other fish. The calculating board produced by Lea for the herring or arrangements based on the same principles are used.

The paper slips used by the "growth measurements" are numbered in such a

manner that it is possible later on to refer from the tabulation to each individual scale on the slide to check the measurements if necessary.

Young fish sampling. Special stress is laid on getting samples of the young, about six months old, sprat in the autumn. These studies have shown that the length of the young fish may vary from year to year, often even from school to school and from fiord to fiord, and, generally speaking, in such a way that the young fish and the older sprat as well, are larger the farther north they are found off the west coast. This has been supposed to be the result of earlier or later spawning and the drift of the young fish by the current. The study of these growth variations are thus supposed to give indications of the strength of the current especially during the preceding autumn and of the conditions prevailing during the spawning. Good currents will give a strong impetus to the northward movements of the young sprat and good penetration into the fiord systems, and early spawning and successful hatching of the same will give a large sprat and vice versa.

In this connection the study of the mixed catches — sprats and small herrings together — is of much interest. The sprats in the mixed schools are larger than those of the same year-class from pure schools and have certainly originated from an early hatched brood, which as young fish has arrived in our waters, when the young herrings are of almost the same size, though somewhat larger. They mix and form homogeneous schools, which, mostly by the currents, are driven northward, where such schools are most common.

Cruises. Cruises in the coast waters have usually been made in September to get into touch with the immigrating young fish and if possible to ascertain the strength of the immigration and the penetration of the young sprat into the fiord systems. Then cruises have been undertaken in May, before the new season, and during the first part of the fishing season, generally in June.

I have in these remarks included most of the methods used in our sprat investigations, firstly because they have not been published before as a whole and secondly because with the ever changing stock in the Norwegian waters every method may be of use in elucidating the fluctuations in this alternative play. Even the vertebrae counting, especially of the young, half a year old fish, may be of interest in this connection.

As may be seen, the investigations have been carried out in such a way as to make it possible without any delay to get in touch with the new year-class, which is destined to be the basis of the fishery in the following season and to follow the changes in the stock by means of cruises and sampling before and during the fishing season. The investigations are handicapped by the short time between the immigration of the new year-class and the utilization of the same by the fishery. This condition makes reliable forecasts of the future yield very difficult. By attacking the question from different points, as shown above, some results have, however, been arrived at regarding forecasts as well.

As the sprat is a very shortlived species, and the spawning seems to take a heavy toll of the stock, the magnitude of the year-classes should best be studied during the

first 2 years. As, however, the Norwegian sprat stock is largely dependant on immigration from other areas, the strength of which fluctuates with the currents along the coast, the real magnitude of a year-class as a whole can not be studied in those waters. Then we have the difficulty that there is generally only one year-class present and there is no opportunity for comparison with younger or old year-classes.

During the 3rd summer the sprat on the Norwegian west coast habitate the outer coast waters, in and out of the "Skjergård", and is seldom fished for, but is often found in whales and coalfish. Some of the year-class become mature and spawn.

During the 4th spring the greater number of the year-class becomes mature and migrates to the spawning places to spawn. At this biological stage there might perhaps be an opportunity to compare the strength of the year-classes, as several year-classes might be supposed to congregate at the spawning places. My opinion, however, is that the spawning process of the sprat takes a heavier toll of the individuals than, say, that of the herring, and that a reliable estimation of the real strength of the year-classes is not possible. This question I may, however, best leave to those of my colleagues, who have more experience in the study of spawning sprats. Investigation into the mortality and death-rate of the sprat seems to me to be a very difficult question as there, to my knowledge, is no pronounced fishery on a stock of more complex character, with several year-classes represented in sufficient numbers for such calculation. Perhaps I am prejudiced in this direction by my working under the special conditions of the Norwegian sprat stock. But this I must, however, leave my colleagues in the sprat investigation to decide.

VI. SARDINE

L'état actuel de la notion de „Races" chez la Sardine.

Par

Jean le Gall.

La sardine semble un bien petit poisson pour avoir oser attirer l'attention de compétences scientifiques que pourrait tenter davantage l'étude de poissons de plus grande taille tels le Hareng, la Morue, le Haddock, le Merlu, la Plie, et sur lesquels se concentre toute l'activité de la Grande Pêche ou des pêcheries hauturières des nations maritimes européennes.

Cependant, il n'en est pas moins vrai que la pêche de ce petit Clupéide, par la façon même dont elle est pratiquée à proximité des côtes, et par les industries connexes qu'elle alimente, est, depuis plusieurs centaines d'années déjà, d'une importance économique réelle pour les populations côtières d'Espagne, du Portugal, de France et du Sud de l'Angleterre.

Dans chacun de ces pays intéressés par cette pêche, il y eût, à différentes reprises dans le passé, une véritable »Question Sardinière«, question d'ordre purement économique d'abord, mais qui, soumise aux Biologistes vers la fin du siècle dernier, devint, dès lors, d'ordre scientifique.

Nous devons ainsi aux travaux publiés à cette époque par: Pouchet, Marion, A. Odin, Cunningham, Fabre-Domergue et Bietrix, nos premières connaissances sur la biologie de la Sardine; notions précisées ensuite par les recherches de Dunn, Cepede, Sauvage, Cligny, Bounhiol et Hoeck, dans la première décade du 20ème siècle. Le mérite revient ensuite à Fage d'avoir, pour la première fois, exposé d'une façon bien nette le problème des races de sardines qui nous intéresse aujourd'hui, et, établi en grande partie, les bases de la technique employée encore à l'heure actuelle par la majorité des Scientifiques de toutes les nations attachés à résoudre ce problème complexe de la biologie de la sardine et la question non moins importante de Races ou de Variétés qui s'y rattache.

L'examen critique des méthodes de travail mises en œuvre dans les travaux récemment publiés, soit en Espagne ou au Portugal, soit en France, nous permettra de préciser l'état actuel du problème et de formuler, conformément aux décisions du Conseil Permanent International pour l'Exploration de la Mer, les bases possibles d'un plan d'études en commun, dont l'application pourra nous donner l'espoir d'apporter, quelques nouvelles lumières sur la biologie encore obscure de la Sardine.

I. L'Examen des Statistiques.

Les quantités de sardines (exprimées en poids) débarquées dans les différentes régions de pêche ont fait, dans chaque pays, l'objet de Statistiques Mensuelles et Annuelles.

L'examen suivi des Statistiques Mensuelles a permis d'établir, pour chaque région, l'époque approximative des maxima de pêche, c'est à dire des grandes concentrations de sardines dans le voisinage des côtes.

Il apparaît ainsi que, si la sardine peut se rencontrer à toute époque de l'année sur les côtes atlantiques d'Espagne, du Portugal, de France, ou sur la côte Sud d'Angleterre, des concentrations bien plus importantes se font assez régulièrement à des époques déterminées:

de Juin à Octobre, sur la Côte Sud d'Angleterre,
de Juin à Novembre, sur les côtes Nord du Golfe de Gascogne,
d'Août à Avril, sur les côtes Sud de ce Golfe,
de Mai à Janvier, dans la région de Santander,
d'Avril à Décembre, dans la région de Vigo,
de Juillet à Octobre, vers Isla Cristina,
de Juillet à Janvier, vers Setubal,
et de Juin à Octobre, à Areiro.

L'interprètation des Statistiques Annuelles tentée à diverses reprises, avec l'espoir évident de retrouver dans les fluctuations quantitatives annuelles de la pêche de la sardine, un rythme régulier pouvant donner une explication à ces fluctuations elles mêmes, et prévoir les rendements ultérieurs possibles de cette pêche, n'a pas donné de résultats bien tangibles.

Ces résultats seraient d'ailleurs douteux, car, il importe de faire certaines réserves sur la valeur de ces statistiques comme documents scientifiques.

Nous avons déjà signalé qu'il y a toujours eu, et il y aura pendant longtemps encore, une Question Sardinière d'ordre purement économique. Or les éléments mêmes de cette Question sardinière:

rapports des pêcheurs avec les producteurs de rogue,
rapports des pêcheurs avec les usiniers conserveurs,
rapports des usiniers avec leur personnel,
rapports des pêcheurs entre eux,

sont autant de facteurs qui, liés au facteur météorologique, *peuvent influer grandement sur le rendement de la pêche*, ceci indépendamment des autres facteurs biologiques ou hydrologiques. Il est donc impossible d'attribuer à ces deux derniers facteurs seuls, les fluctuations quantitatives relevées dans les statistiques annuelles ou même mensuelles, et, *tant qu'il ne sera pas possible de préciser dans les statistiques elles mêmes, l'influence des facteurs d'ordre économique sur le rendement de la pêche, l'examen de ces statistiques ne pourra se faire qu'avec la plus grande circonspection et leur valeur scientifique restera toujours douteuse.*

Le problème des Races de sardines étant posé, le seul moyen de définir ces races avec précision était d'établir sous forme d'Indices la valeur numérique de caractères morphologiques bien définis et calculés sur le plus grand nombre possible d'exemplaires.

En majorité, les Auteurs actuels ont repris pour la Sardine les caractéristiques morphologiques définies par Heincke pour le Hareng; à savoir: les différentes proportions du corps rapportées à la longueur totale de l'individu, proportions relativement stables dès que le poisson a atteint sa forme définitive; puis, le nombre de rayons des nageoires, et enfin, le nombre de vertèbres présenté sous forme de moyenne vertébrale.

Les différentes proportions ainsi calculées ont été les suivantes:

Di: Longueur totale du corps divisée par la distance du bout du museau à l'extrémité antérieure de la nageoire dorsale.

Vi: Longueur totale du corps divisée par la distance du bout du museau à l'extrémité antérieure des nageoires ventrales.

Ai: Longueur totale du corps divisée par la distance du bout du museau à l'anus.

An: Longueur de la nageoire anale divisée par la longueur du corps.

Ds: Longueur de la nageoire dorsale divisée par la longueur du corps.

lcpl: Longueur latérale de la tête, mesurée du bout du museau au bord postérieur de l'opercule, divisée par la longueur du corps.

Elles ont donné les résultats ci-joints, condensés sous forme de tableau.

Origine du matériel	Di	Vi	Ai	100 An	100 Ds	100 lcpl	Auteurs
Manche: Boulogne	2,67	2,33	1,60	12,3	9,6	19,6	Le Gall.
Manche: Plymouth	2,67	2,38	1,60	11,6	10,6	20,2	Le Gall.
Atlantique: Audierne	2,60	2,27	1,57	12,5	10,5	20,8	Le Gall.
— Douarnenez	2,68	2,27	1,57	12,1	11,1	19,9	Le Gall.
— Concarneau	2,69	2,40	1,59	12,4	10,4	20,1	Le Gall.
— Quiberon	2,63	2,35	1,54	12,9	9,5	19,7	Priol.
Sables d'Olonne	2,71	2,38	1,62	11,9	10,5	20,5	Desbrosses.
La Rochelle	2,72	2,41	1,61	11,9	9,6	18,5	Belloc.
St. Jean de Luz	2,67-2,70	2,39	1,60	14,3	12,7	20,8	Arne.
Golfe de Gascogne	2,65	..	..	..	..	20,5-21,1	Fage.
Sétubal	2,60	..	..	..	..	22,2	Fage.
Vigo	2,65	..	1,55	..	..	..	De Buen.
Portugal	2,60	..	..	..	..	20,7	Ramalho.

Bien que ce tableau soit incomplet et ne permette pas de suivre les variations d'un des caractères déterminés, des côtes de la Manche au Portugal, il est possible cependant de constater que *l'ordre de grandeur de la variation des caractères envisagés reste faible* (exception faite des caractères: Longueur de l'anale et de la dorsale, nettement supérieurs à Saint Jean de Luz et qui n'ont pas été calculés au Sud de cette région); *il ne dépasse pas l'ordre de grandeur des erreurs techniques possibles:* erreurs systématiques inhérentes à une technique encore imprécise, erreurs accidentelles dépendant de l'opérateur et des conditions de mesure.

Il serait donc prématuré de conclure sur la validité de ces caractères comme caractères distinctifs des races possibles de sardines.

Le nombre de Rayons de la Nageoire Dorsale n'a été compté que par deux auteurs: Fage et Arne. Ce caractère semble peu variable; mais, il convient cependant de signaler les résultats différents obtenus par ces deux auteurs dans deux régions voisines:

Arcachon (Fage) 17,86 (52 exemplaires étudiés).

St. Jean de Luz (Arne) 18,41 (174 exemplaires étudiés).

Ces observations sont en nombre insuffisant pour qu'une conclusion puisse en être tirée.

Le calcul de la Moyenne Vertébrale a, davantage, attiré l'attention des Chercheurs et les résultats obtenus sont intéressants.

Origine du matériel	Auteur	Moyenne vertébrale et Fluctuation probable de la moyenne	Nombre examiné	Observations
Plymouth	Fage	52,25 ± 0,22	185	après Heincke
Sardines de la Manche	Le Gall	52,23 ± 0,10	592	
Locquémeau	Le Gall	52,23 ± 0,21	130	Manche
Brest	Fage	51,78 ± 0,20	111	
Douarnenez	Le Gall	52,22 ± 0,13	277	
Audierne	Le Gall	52,19 ± 0,16	79	
Penmarch—Concarneau	Le Gall—Priol	52,23 ± 0,15	146	
Concarneau et Guilvinec	Le Gall	51,91 ± 0,24	79	Sardines dites de dérive
Quiberon	Priol	52,31 ± 0,18	158	
Manche plus Côte Sud de Bretagne	Le Gall	52,24 ± 0,06	1487	
Côte Sud de Bretagne	Le Gall	52,24 ± 0,07	895	
Sables d'Olonne	Desbrosses	52,31 ± 0,10	202	
La Rochelle	Belloc	52,23 ± 0,10	385	
La Rochelle	Fage	51,88 ± 0,24	90	
Arcachon	Fage	51,74 ± 0,17	186	
St. Jean de Luz	Arne	51,39 ± 0,12	372	51,34—51,44
Santander	Fage	51,69 ± 0,19	134	
Cadix	Fage	51,30 ± 0,27	84	
Sétubal	Fage	51,31 ± 0,21	106	Moyen. des deux 51,31 ± 0,17
Portugal Nord au Sud	Ramalho	51,24 ± 0,03	2837	Résultats généraux
Açores	Fage	51,95 ± 0,20	95	
Açores	Ramalho	51,64 ± 0,14	155	
Madère	Ramalho	51,83 ± 0,23	47	

Le coefficient d'erreur technique, vraisemblablement limité ici aux erreurs accidentelles pour un observateur habitué et attentif, *est*, pour ces différentes moyennes verté-

brales, *inférieur à la variation observée entre les sardines de la Manche, du Nord du Golfe de Gascogne et les sardines du Sud du Golfe de Gascogne, des côtes du Portugal et d'Espagne: le caractère est donc utilisable.* Il est ainsi possible d'admettre l'existence probable de deux races de sardines sur la côte Atlantique d'Europe.

1) Une race localisée sur les côtes de Bretagne (au Nord du Golfe de Gascogne) et sur les côtes de la Manche. Race caractérisée par une moyenne vertébrale supérieure à 52 et voisine de 52,24.
2) Une race localisée au Sud du Golfe de Gascogne et sur les côtes d'Espagne et du Portugal, caractérisée par une moyenne vertébrale inférieure à 51,50 et voisine de 51,30.

Il existe en outre vraisemblablement une race atlantique, localisée dans les parages de Madère et des Açores et dont la moyenne vertébrale est comprise entre 51,50 et 52.

La limite d'extension, dans le Golfe de Gascogne, de chacune des deux races précédemment définies reste encore imprécise. Il est d'ailleurs probable qu'elle doit varier suivant les conditions de milieu et l'état physiologique des individus de chacune d'elle.

L'examen des caractères biologiques et l'étude systématique de la variation de ces caractères en fonction du temps, ont permis de préciser, dans une certaine mesure, la notion de deux races distinctes de sardines sur notre côte atlantique, en tâchant de suivre chacune de ces races dans son évolution respective. Cette étude, encore incomplète, a surtout été entreprise en Manche et dans le Nord du Golfe de Gascogne.[1]) Les caractères biologiques étudiés ont été les suivants:

Taille, mesurée en centimètres et rapportée au nombre entier le plus voisin.
Poids, évalué en grammes.
Age et croissance, calculée d'après l'écaille.
Sexe et maturité. Les stades de I à VIII préconisés par J. Hjort pour le hareng ayant été appliqués, avec la même valeur, à l'étude de la sardine.
Engraissement périodique. La quantité de matière grasse présente dans les mésentères intestinaux ayant été évaluée de I à V suivant son abondance.

Taille.

Un seul fait bien net ressort de l'étude de ce caractère: En Manche, les sardines de taille moyenne: immatures de 10 à 15 centimètres sont rares; la grosse majorité de la population sardinière de la Manche étant constituée par de grands individus adultes de 18 à 25 centimètres, avec une forte proportion d'individus de 20 à 23 centimètres. Les sardinettes, ou individus de l'année, se rencontrent cependant dans cette mer; ils disparaissent dès les premiers froids, leur taille atteignant alors de 8 à 11 centimètres.

[1]) Etude générale entreprise sur les côtes françaises en 1929 par l'»Office Scientifique et Technique des Pêches Maritimes«. En cours de publication.

Dans le Nord du Golfe de Gascogne (Côtes de Vendée et de Bretagne) les grands rassemblements estivaux de sardines sont constitués par des individus de 11 à 15 centimètres. Des sardines de grande taille se rencontrent également à cette époque mais en petite quantité. Elles sont, au contraire plus abondantes au moment de la pêche de la sardine »dite de dérive« (de Mars à Juin): les sardines de 11 à 15 centimètres n'apparaissant pas, à ce moment, dans la zone côtière.

La pêche se pratiquant dans cette région à l'aide de filets dérivants à mailles d'une grandeur déterminée (maille qui définit le "moule" du poisson) on serait tenté de croire que l'emploi de ces engins opère une sélection parmi les individus présents sur les fonds de pêche. Des observations régulièrement suivies ont permis de vérifier que cette sélection opérée par les filets est néanmoins nettement représentative de la population sardinière de la région.

L'apparition de sardinettes (individus de 8 à 11 centimètres) a été signalée à différentes reprises, de Septembre à Novembre, dans la région côtière du Nord du Golfe de Gascogne.

Dans le Sud du Golfe de Gascogne et sur les côtes d'Espagne et du Portugal, l'emploi habituel de sennes pour la pêche permet la capture d'individus de toutes les tailles pendant toute l'année. Il apparaît cependant que les individus de 11 à 15 centimètres dominent encore dans la région côtière, et, que les individus plus grands (vraisemblablement les reproducteurs) n'apparaissent en grand nombre qu'en certaines régions et à certaines époques qu'il serait intéressant de préciser.

Poids.

La notion de poids n'est pas exempte de critique au point de vue de sa valeur comme caractéristique biologique.

Le poids moyen d'une sardine normale est de 25 à 30 grammes; or, en remarquant que la sardine est habituellement attirée vers les engins de pêche au moyen d'un appât dont elle se gave, on se rend immédiatement compte de ce que peuvent avoir comme importance les variations de poids du contenu stomacal du poisson dans l'établissement du poids moyen de l'individu.

Néanmoins, la notion de poids, ou plus exactement de *poids moyen*, c'est à dire: nombre de sardines au Kilogramme, a été retenue dans l'étude des sardines de la côte de Bretagne et pourrait être étendue vraisemblablement à l'étude des sardines des autres régions. Les relevés journaliers des principaux ports sardiniers français indiquent, en effet, très fréquemment, le »moule« du poisson, moule établi d'après le nombre de poissons au Kilogramme, ou d'après la grandeur de la maille des filets employés à sa capture. Des observations suivies et fréquemment vérifiées ont permis de faire correspondre cette notion de »moule« du poisson, notion basée sur le poids moyen des individus ou sur la connaissance de l'engin de capture, à une notion suffisamment précise de taille et d'âge des individus pour se rendre compte de la composition principale de la population sardinière de la côte de Bretagne. *Cette notion serait avantageusement étendue aux autres régions de pêche à la sardine.*

Age et Croissance.

Les techniques scalimétriques courantes appliquées à la sardine ont permis d'établir l'âge des sardines fréquentant les diverses régions de pêche étudiées; et, il est actuellement nettement établi:

que *les Sardinettes sont des sardines de l'année, sans trace d'anneaux d'hiver sur l'écaille et d'une taille généralement inférieure à 11 centimètres;*

que *les individus de 11 à 16 centimètres, les plus abondants sur les pêcheries côtières, ont en général un an révolu et pas encore deux ans (un seul anneau d'hiver) et que les individus plus grands sont plus âgés.*

En anticipant sur la notion de maturité sexuelle, il est possible d'ajouter que les individus les plus abondants sur les pêcheries sont encore immatures: la sardine n'étant, le plus souvent, en état de se reproduire qu'après sa deuxième année d'existence (avec plus de deux anneaux d'hiver).

L'âge et la taille correspondante étant connus, la notion de Croissance (c'est à dire: l'augmentation de taille pendant l'unité de temps admise: l'année), s'en déduit aisément. Cette notion a fait l'objet de nombreux travaux; et, en se rapportant uniquement au travail de Fage et au rapport ultérieur de Le Danois, on constate que cette notion de croissance permet de *distinguer immédiatement les sardines de la Manche à croissance continue après la seconde année, des sardines du Golfe et de la côte d'Espagne et du Portugal dont la croissance est ralentie à partir de cette deuxième année.*

Il nous paraît toutefois indispensable, pour éviter à l'avenir toute confusion dans l'interprétation des différentes valeurs de ce facteur »croissance« de définir avec précision la façon de le calculer.

Ce facteur doit être uniquement établi d'après les tailles calculées au moment de la formation des différents anneaux d'hiver et non d'après les tailles observées[1]*, à des moments quelconques, sur des individus portant le nombre correspondant d'anneaux d'hiver.*

Sexe et Maturité.

Quelle que soit la région envisagée, il ne semble pas que la sardine se reproduise, pour la première fois, avant l'âge de deux ans révolus: taille supérieure à 15 centimètres. L'élaboration des glandes génitales est lente, leur maturation assez rapide. Elle ne se produit pas simultanément chez tous les individus adultes de la même région et il en découle que la période de ponte se poursuit pendant plusieurs mois.

F. de Buen a mis en évidence que: sur les côtes d'Espagne,

d'Avril à Août, l'état sexuel des jeunes sardines est à peine indiqué, et que chez les adultes les organes génitaux sont en période d'évolution lente.

de Septembre à Octobre, la maturation s'achève rapidement et que la ponte se poursuit ensuite de Novembre à Mars.

[1]) Taille réelle des individus observés.

Dans la région Nord du Golfe de Gascogne et en Manche, la même période d'évolution lente se retrouve de Juillet à Janvier, suivie d'une période de maturation rapide de Février à Mars, la ponte se faisant normalement de Avril à Juin[1]).

L'emploi généralisé des Stades préconisés par Hjort *pour déterminer les différents états de maturité du Hareng serait avantageusement applicable à la sardine et permettrait de suivre d'une façon plus précise l'évolution génitale de ce poisson dans chacune des régions considérées.*

Engraissement Périodique.

Les informations recueillies sur ce point sont encore insuffisantes pour en tirer une conclusion générale. Il semble cependant qu'à la période d'évolution génitale lente corresponde, dans chaque région, une période d'engraissement, et que, les graisses mises en réserve pendant cette période disparaissent ensuite rapidement pendant l'époque de maturation rapide, avant la ponte.

EN RÉSUMÉ: dans l'état actuel de nos connaissances sur la biologie de la sardine, il est possible d'admettre que deux races distinctes se répartissent sur le littoral européen, du Sud de l'Espagne à l'entrée Sud de la Mer du Nord:

Une race méridionale, rencontrée sur les côtes d'Espagne et du Portugal et dans le Sud du Golfe de Gascogne, caractérisée par une moyenne vertébrale inférieure à 51,50.

Une race septentrionale, retrouvée dans le Nord du Golfe de Gascogne et en Manche, caractérisée par une moyenne vertébrale supérieure à 52.

Dans l'état actuel de nos connaissances, il n'est pas encore possible de subdiviser des deux races en variétés locales nettement définies.

La limite d'extension de chacune de ces deux races dans le Golfe de Gascogne reste encore indéterminée. Il est possible de la supposer comme étant variable suivant les conditions hydrologiques du Golfe et l'état physiologique des individus de chacune d'elle.

L'évolution de chacune de ces deux races, dans son domaine particulier, apparaît comme identique. Elle peut se résumer ainsi:

La ponte a lieu à quelque distance de la côte. Elle se prolonge pendant plusieurs mois et succède à une période d'élaboration génitale lente suivie d'une maturation rapide. Les reproducteurs sont généralement âgés de plus de deux ans et leur taille est alors supérieure à 15 centimètres.

Les larves écloses rejoignent le littoral et fréquentent la zone littorale pendant la première partie de leur existence. Elles s'écartent normalement de la côte (sardinettes) avant la fin de leur première année, puis y reviennent âgées d'un an révolu. Ce sont les

[1]) d'aprés Ramalho, la ponte de la sardine méridionale se poursuivrait également, au sud du 40° du latitude Nord, pendant le deuxième trimestre.

»immatures« attirées dans la zone côtière par l'abondance de nourriture (concentration de nutrition). Elles s'en écartent encore, un peu plus tard, pour réapparaître l'année suivante, agées de deux ans révolus, sur les frayères (concentration de reproduction).

Après la ponte, les individus adultes peuvent encore se retrouver dans la zone côtière, mais en moins grand nombre que les immatures et en se tenant plus au large. Chaque année, ensuite, ils s'écarteront régulièrement, à certaines époques, de cette zone côtière pour reparaître quelque temps plus tard sur les frayères.

Quelques caractères biologiques particuliers permettent encore de distinguer ces deux races au cours de leur évolution:

Ce sont:

1) La croissance: lente à partir de la deuxième année pour la race méridionale, plus rapide dans le Nord, particulièrement pour les sardines de la Manche.
2) L'état sexuel des individus adultes: en général plus avancé dans le Sud que dans le Nord, la ponte étant terminée pour la race méridionale quand elle débute pour la race septentrionale[1]). Cette notion demande toutefois à être précisée, en appliquant aux différents stades de maturité de la sardine les valeurs numériques précisées par Hjort, avant d'être considérée comme définitive.

L'influence des conditions hydrologiques sur l'évolution et les déplacements de chacune de ces deux races reste encore imprécise.

Il apparaît clairement, cependant, que le facteur température a une influence marquée; et, bien que les résultats obtenus soient encore incomplets, il semble acquis pour chacune des deux races que:

la ponte s'effectue dans les eaux de température de 11 à 15 degrés;
l'élaboration génitale, correspondant à la période de concentration de nutrition dans les eaux côtières, se fait au moment où ces eaux présentent leur température maximum. Il apparaît de plus, qu'elles s'écartent de ces eaux quand leur température vient à baisser.

L'influence du facteur salinité reste encore à étudier.

En conclusion, nos connaissances sur la biologie de la Sardine sont bien incomplètes et les quelques notions actuellement acquises demandent encore à être précisées pour pouvoir être admises avec certitude.

Il ne saurait donc pour le moment être question de fluctuations, de taux de mortalité et de prévisions sur l'avenir de la pêche, quand les éléments de base de cette étude restent encore incertains.

Il nous apparaît que cette incertitude des résultats acquis provient surtout du manque de collaboration, d'unité de méthode dans le travail.

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[1]) voir note précédente.

Des Scientifiques, dans chaque pays, ont fouillé longuement cette question, la traitant minutieusement point par point, mais, suivant des méthodes particulières et souvent différentes entre elles. Les résultats obtenus, quoique de valeur, ne sont malheureusement pas toujours comparables scientifiquement.

Suivant les fortes paroles du Professeur J. Hjort »Il faut en finir avec les difficultés«, toutes ces méthodes particulières doivent disparaître devant une méthode unique de travail élaborée en commun. Aux petites écoles nationales doit faire place l'unique école internationale.

L'organisation d'une coopération régulière constante, basée sur un plan de travail commun est devenue indispensable. Ce plan dont les détails ne sauraient être arrêtés qu'après discussion entre les Scientifiques de chaque pays attachés à l'étude de la Sardine, pourrait être établi, dans ses grandes lignes, sur le plan adopté d'une facon générale pour l'étude du Hareng. Nous le résumerons ainsi:

1) Régions de pêche.
   Limites de chaque région. Epoques de pêches et fluctuations des pêcheries sous l'influence des différents facteurs. Conditions hydrologiques dans chaque région.
2) Prélèvement du matériel et étude de ce matériel:
   Conditions du prélèvement et nombre d'exemplaires à étudier.
3) Caractéristiques morphologiques:
   Discussion des caractères à étudier et des caractères à éliminer. Etablissement d'une technique précise de mensurations. Interprétation mathématique des résultats.
4) Caractéristiques biologiques:
   Taille. Age et croissance (Technique scalimétrique à préciser). Sexe et maturité. (Application des Stades définis par J. Hjort.) Engraissement périodique.

Variations de ces phénomènes biologiques en fonction du temps, dans chaque région.

Ce plan incomplet fait appel aux suggestions des différents Observateurs. Il ne peut constituer que le schéma d'un plan définitif de travail. Les résultats dernièrement acquis, dans son application à l'étude du Hareng, peuvent nous donner l'espoir qu'une collaboration étroite et suivie mise en œuvre à l'étude de la sardine, nous permettra de préciser d'une façon définitive les quelques connaissances déjà acquises, et d'élucider le problème encore bien complexe qui nous est posé.

DISCUSSION

Dr. **Ed. le Danois.** — Les résultats qui ont été exposés par M. LE GALL sont en grande partie le fruit de ses observations personnelles et d'une enquête générale entreprise sous ma direction par le personnel de l'Office Scientifique et Technique des Pêches Maritimes.

J'avais, en effet, divisé la côte française depuis le Finistère jusqu'à la frontière espagnole en 5 secteurs et, à la même époque, c'est-à-dire, vers le 10 Juillet 1929, cinq délégués de l'Office des Pêches: MM. LE GALL, PRIOL, DESBROSSES, BELLOC et ARNÉ se livrèrent à des observations identiques tant au point de vue ichthyométrique qu'au point de vue biologique sur les races de sardines peuplant le Golfe de Gascogne.

Il apparaît nettement et clairement que deux races coexistent dans cette région: l'une au sud, dans la zone de St-Jean-de-Luz et Arcachon qui peut être caractérisée par une formule vertébrale inférieure à 52 vertébres; l'autre au nord, mais cette dernière semble comprendre des populations diverses; leur formule vertébrale indiquant plus de 52 vertébres avec des variations qui sont insuffisantes pour déterminer exactement diverses races locales. Ces sardines s'allient étroitement à celles qui vivent plus au nord, particulièrement en Manche.

Dans toute cette région le problème reste donc assez obscur et en qualité de Président du Comité du Plateau Continental Atlantique, j'ai l'intention de demander au Bureau du Conseil de bien vouloir provoquer une réunion d'experts dans un des pays représentés dans le Comité; ces experts se mettraient d'accord sur la technique à suivre, car, encore à l'heure actuelle, celle-ci semble assez mal définie.